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THE SHIBBOLETHS OF TUBERCULOSIS

SHIBBOLETH :

"A catch phrase or cry to which the members of a party adhere after any significance or meaning which it may have imparted has disappeared."

Ency. Brit.

"Christians were ready to insist on that insensate shibboleth: Except ye be circumcised, ye cannot be saved."

H. R. Reynolds: *John the Baptist.*

THE SHIBBOLETHS OF TUBERCULOSIS

BY

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PREFACE

THERE are many statements accepted by the medical profession concerning pulmonary tuberculosis which are either erroneous or misleading. These traditions are conveyed rather by word of mouth than by books. Some exist as ideas, and hardly ever reach expression in definite terms. Many of these statements I have heard repeated, over and over again, by members of our profession who have visited me at the various institutions with which I have been associated. If progress is to be made in the treatment and eradication of tuberculosis, the sooner these false doctrines are corrected, the faster will be the progress. It is to rectify these misstatements and hasten that progress that I have written this book.

There is ample time for the reconstruction of the system of education concerning most of these misconceptions. There is, however, a pressing danger of a fatal step being taken with regard to the provision of what are termed Farm Colonies. I hope what I have written will be considered before the authorities concerned embark upon such schemes.

I am much indebted to Dr Arcangelo Distaso for conducting the inoculation experiments; to

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my old Frimley comrades, Capt. Inman, for the opsonic charts; Surg.-Comd. Atkinson, D.S.O.; Surg.-Lieut. Blandford; Capt. Harries, and Drs Sutcliffe and Watson; and also to Major Russell Jones for reading the manuscript and making helpful suggestions.

My thanks are due to Mr E. W. Richardson for seeing the book through the press.

HENDON, *Easter* 1920.

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THE SHIBBOLETHS OF TUBERCULOSIS

I

THAT IT IS GOOD TO STERILISE MILK TO LIMIT THE SPREAD OF TUBERCULOSIS

If to this statement, the words "and its Products" were added after "milk," it would appear to be absolutely sound, from a tuberculosis point of view. Is it not acting like the ostrich, to deal with the fluid milk and leave out of account the solids—butter and cheese, both equally capable of spreading tuberculosis? Is it a sign of mental balance to boil milk for growing children to prevent tuberculosis, and at the same time to feed them on unboiled butter and cheese? One can only suppose that milk is selected for special attention, because of the impossibility of boiling butter and cheese. The International Red Cross Convention on After War Work, sitting at Cannes in April 1919, suggested the sterilisation of milk. It apparently did not consider the products, butter and cheese.

There is a great want of proportion in the way the milk question is regarded. The facts are, that

milk, and therefore its products, are considered capable of communicating tuberculosis to mankind. Great Britain could, if she so desired, control and obtain a tuberculosis-free milk, either by Bang's methods (to be referred to later in detail), or by sterilising the milk. It could *not*, however, control the manufacture of the butter and cheese, because these are supplied from all over the world. The shortage of both during the war, owing to lack of transport, should make this fact easily recognisable.

Not only do the tubercle bacilli exist in butter, but they can live in it for a long time. The United States Department of Agriculture[1] published statistics to show that they had examined 775 samples of butter, and that 12 per cent. of these specimens were found to be capable of producing tuberculosis. They conducted animal experiments with the infected supplies, and found that, even in heavily salted butter, the tubercle bacilli remained virulent for *at least* ninety-nine days. Newman publishes a table showing that for 498 specimens of butter tested on the Continent, bacilli were present in 15·2 per cent.

Harris in Islington[2] found tubercle bacilli present in 14·4 per cent. of the samples of milk he investigated, and Bang[2] 14 per cent., so that the risk from butter is probably much the same as that from milk.

Tubercle bacilli have frequently been found in cheese.[3] Galtier's experiments with cheese, both salted and unsalted, demonstrated that the bacilli remained virulent for over two months. The

U.S.A. Public Health Laboratory[4] conducted tests which demonstrated that the virulency of tuberculous cheese lasted for 104 days.

There is no reason why an attempt to follow Bang should not be made in this country, and so make the purchase of both non-tuberculous butter and cheese possible. In the meantime, the only really sensible course for the advocates of sterilised milk to adopt is to sterilise the milk, and to give the children dripping and margarine, and forbid the eating of cheese unless it is four months old. Margarine alone is not sufficient, as it does not contain the vitamines found in butter.*

There is quite a large body of opinion to the effect that we acquire, during our infancy, a certain immunity to tuberculosis from the constant ingestion of small infections through milk. That we shall certainly lose this immunity if we adopt the measures suggested, is obvious from this point of view.

If we would, as a nation, control the sources of infections, *i.e.*, sputum and milk products, I should strongly advocate that control, and altogether neglect this haphazard, uncontrolled source of possible immunisation.

One other thought on milk products. Here is a practically unrecognised method of contracting tuberculosis, and yet, when a case of tuberculosis is diagnosed in a family, the parents are indignant; they look upon the suggestion as a slur on their family. They say, "there is none in the family. Our parents and grandfathers all died of old age;

* Cod-liver oil also contains these fat, soluble vitamines.

all our relatives are strong and healthy, and there is no trace of it in the family." Careful investigation might have found the "trace of it" in the butter or the cheese.

II

THAT THE PROVISION OF SPUTUM FLASKS FOR PERSONS WITH TUBERCLE BACILLI IN THEIR SPUTUM, WILL LARGELY PREVENT TUBERCULOSIS

It certainly would help greatly, if it were not for the "man in the street." In spite of all the educational efforts which have been made to bring home to the public the principal facts concerning tuberculosis, there is still a want of their proper appreciation. The statement that tuberculosis is a disease which one human being can communicate to another, has deeply taken root. The knowledge that a person suffering from tuberculosis need not be a source of infection to anyone, if certain simple precautions are taken, has not been grasped. It is not sufficiently recognised that sputum can be collected without danger and rendered inert.

Here, I cannot do better than quote a paragraph from a report I made in 1915 for the King Edward VII. Welsh National Memorial Association[5]:—

"Every person suffering from tuberculosis who is allowed to go about his or her daily occupation, should carry a sputum flask. Each person diagnosed as suffering from pulmonary

tuberculosis has been supplied with a sputum flask. Let every person who reads this part of the report endeavour to recollect how many of these flasks he has seen in use, either in a railway train, tramcar, or other place. There is no doubt the flasks are used at home to protect the families. Equally, there is no difficulty in institutions in persuading the patients always to spit into flasks. Why, therefore, when they return to the towns and villages, will they not use them in public? The answer is simple enough. Their use is easy in a sanatorium or hospital, because those persons who do not need to use flasks are sympathetic towards their employment by the persons afflicted with tuberculosis, and nothing but encouragement is given to the patients to make every possible use of these collectors . . . Yet what is the picture that we see when the tuberculous person mixes with the public? The patient who is careless, either through ignorance or indifference, to the spread of the disease to his fellow-creatures, whether in a public conveyance, public house, theatre, or cinema, may spit promiscuously, and he will be tolerated, although probably not liked. Let, however, a patient who has been properly trained in the collection of the bacilli infected sputum take out his sputum flask in a railway carriage and everyone will flee from him. In fact, it is practically only necessary to carry a sputum flask and exhibit it, to have a railway carriage to oneself. This would almost seem humorous if it were not so very serious. It means that those patients who are willing to collect and properly dispose of the seed are to be discouraged in every way from carrying out what they know to be right when mixing with their fellow-creatures. Patients soon fail to see any reason to advertise that they suffer from

tuberculosis, if in consequence they are to be looked upon as lepers. To use a sputum flask in the present state of public opinion may mean the loss of a man's job."

The same applies to-day (April, 1920).

I continued—"Until the time comes when the 'man in the street' realises that he is safer with the person having pulmonary discharges who expectorates into a flask, than with one without such precaution, so long will the spread of the disease be continued. These are stern facts. Tuberculosis is an invisible foe. The person capable of spreading infection, can keep the knowledge to himself and spread the disease. What is the reward of doing his duty to his fellows?"

Yet people still wonder how they can possibly have contracted this disease! Much is spoken at congresses on the control of tuberculosis, and still more written in the medical press, but never have I read of this matter being thrashed out. After all that has been said and written, here we remain, in 1920, with two of the known main sources of infection—sputum, and milk and its products, practically unchecked, and in too many instances not even realised.

As a child, I was told that lunatics were tried before being released in the following way:— The testing room was a large one with a stone floor. Before the trial, the attendant turned on a water-tap to flood the room. The patient was then admitted, given a broom and told to sweep up the water. If he did not first turn off

the water-tap, he was not given his freedom. Until we check the indiscriminate sowing of the seeds of tuberculosis by solving the problem of sputum collection, do we not resemble the mentally deficient patient who fails to observe the tap?

III

THAT PERSONS WHO HAVE OR HAVE HAD TUBERCULOSIS SHOULD NOT MARRY

This is far too dogmatic a statement. Whether tuberculous persons should marry or not, depends on a variety of circumstances, and each case must be judged on its merits. I will admit that the speediest method of eradicating tuberculosis would be to kill everybody afflicted with the disease. If this is unfeasible, then to prohibit the procreation of the race amongst tuberculous persons might be a good eugenic measure. But we have to deal with conditions as they exist at the present time and not with ideals. It is necessary, therefore, to examine the reasons why this creed obsesses so many minds.

First and foremost is the thought of tuberculous offspring, and secondly, the risk to the conjugal mate. There is no doubt as to certain facts, *viz.* :—

(1) That tuberculous married couples do infect one another;

(2) That they do not always cause disease;

(3) That parents do infect their children;

(4) That parents do not always cause disease in their children;

(5) That tuberculous parents, even when both are tuberculous, do raise healthy families.

It follows, therefore, that even without any precautions, parents do not necessarily cause disease in each other or their children.

The following study of tuberculous families by Hazen,[6] of the Phipp's Institute, illustrates the points enumerated above.

"Eighty-three families, consisting of 534 individuals, were studied under home conditions. The families were selected because of the large number of infections that had occurred in each.

"Of these, 254 were tuberculous, *i.e.*, 47 per cent.; 13 were suspicious, and 267 'apparently well.' Some 43 per cent. of the married patients had apparently infected their consorts. In 48 families there was a diseased parent. In the 48 families were 237 children, and 92 of these developed tuberculosis. In 15 families both parents suffered from tuberculosis, and 'the children are entirely well.' Only three members of the 83 families developed tuberculosis who were not in contact with their families, *i.e.*, less than 1 per cent."

So that, like the calves taken away from the tuberculous cows,* whenever the children were removed from infection, only 1 per cent. developed the disease, and even then their infection is not necessarily due to parental influences.

From the point of view of infection, the conclusions of Verco[7] are also interesting. During his

* See p. 32.

many years' experience as medical referee of the Provident Insurance Company and as chief medical officer for South Australia, he collected statistics on some 500 cases. He comes to this conclusion:—

"Of 100 married women only about 9 should die of phthisis, but if they are married to consumptive husbands, fully 40 will die of phthisis—an increase of 31 in 100. While of 100 married men only 9 should die of phthisis, if they are married to consumptive wives, nearly 12 will die of phthisis—an increase of less than 3 in 100.

"So that phthisis in a husband is very fatal to a wife, whereas in a wife it is only slightly fatal to her husband."

As Verco states, the probability of this higher incidence in the case of the woman is connected with nursing her sick husband. I would go further, and say that the seeds probably began to take root long before the nursing stage was reached; if the husband was untrained in the methods of preventing infection this could hardly be otherwise, as the wife, in the majority of cases, would have to clean the house and handle his bed linen and handkerchiefs. Then, as the husband gradually failed in health and wage-earning capacity, so would the strain to the wife increase, whilst nutrition diminished through lack of wages, until her resistance yielded to the disease.

From a statistical point of view, Karl Pearson and Pope[8] reach the following conclusions after considering reports on 40,000 married couples:—

(1) That there is some sensible but slight infection between married people;

(2) That this is largely obscured or forestalled by the fact of infection from outside sources;

(3) Liability to infection depends on the presence of a necessary predisposition;

(4) Assortitive mating probably accounted for two-thirds, and infective action for not more than one-third of the whole correlation observed in these cases.

Longstaff,[9] after a consideration of the death-rates from tuberculosis, and after an actuarial review, comes to this conclusion: "It is plain, therefore, that to show any substantial argument for the existence of infection, it would require a much larger collection of cases than has yet been published."

I have stated, more than once, that a properly conducted tuberculosis institution need not be a source of danger, and that such a place is as safe as anywhere else for the attendants. There is no reason why two ex-patients should not constitute their home a "tuberculosis institution," in so far as infection to their children is concerned.

I have subsequently recorded that I do not subscribe to the doctrine of transmission of tuberculosis to the fœtus. Therefore, so far as the infants are concerned, the question of marriage should be determined on the points whether or not the parents have been properly trained, and will live up to their education, and also have a healthy environment in which to rear their family.

A certain number of ex-patients have kept in personal touch with me. Many of them I knew had married and raised families. In August 1919,

I inquired of eleven of these married patients who had had bacilli found in their sputum and who had sputum since their discharge, as to whether or not they had infected their wives or families. Their collective answers were as follows :—

None had infected their wives.

Two had no children.

Two had each a healthy daughter.

The remaining seven had forty children, and only one, the youngest of three, showed any evidence of infection.

These patients had all been home for some considerable time.

The infected child need not necessarily have received parental infection. So that the risk to wives and children can be eliminated if the patients are PROPERLY trained.

Certain of these patients volunteered the information that both their wives and families were of far healthier appearance than their neighbours. I can endorse this statement, as I have seen many children reared by ex-patients and the progeny have been a credit to their parents' standard, not of existing, but of LIVING.

With regard to the prospective husband and wife: if they, again, have been, each or either, properly trained, there should be no risk of infection. The possibility of disease would appear not to be great, even amongst untrained couples. By eliminating the source of infection by proper care of sputum and handkerchiefs, the only risk is in their bedroom. Here, with open windows,

separate beds, and proper care, the risk should be only a little greater than that of two patients in a tuberculosis ward; and it has not been suggested that these infect each other.

Finally, I would not consider for a moment the marriage of tuberculous persons unless they fulfilled the following conditions:—

(1) That the tuberculous one had undergone sanatorium treatment, and it was premised that he or she would carry out the lessons therein learned;

(2) That he or she had "arrested" disease, and had remained well for two years after discharge; and

(3) That there was no concealment concerning the disease.

IV

THAT TUBERCULOSIS IS AN HEREDITARY DISEASE

The origin of this supposition is obvious. The children of "consumptive" parents contracted tuberculosis—therefore it followed that it was hereditary. In the days when the statement was first made, it was a very natural conclusion to arrive at. Nothing was then known about the infectivity of sputum. But things have changed, and we should not, parrot-like, perpetuate the sayings of our forebears without due consideration.

There is no doubt that tuberculous parents

can and do rear children free from tuberculosis, and that tuberculous parents do rear tuberculous children. One knows these facts from one's personal experience. There is fortunately, however, plenty of evidence to show that it is far more probable that children of tuberculous parents receive a post-, rather than a pre-natal, infection. The only way to prove the fallacy of the idea would be actual experiments with children, which will be referred to later.

Try to estimate the chances of a child born of a tuberculous mother, or even of a non-tuberculous mother but with a tuberculous father, neither of whom has received any practical instruction in the prevention of infection to themselves or to their offspring. The infant is born into a hot-bed of infection, and remains in it. How can it escape infection? This is the actual condition of affairs which practically obtains almost everywhere to-day.

On the other hand, let us examine Bang's experiments[10] with animals. Professor Bang induced the Danish Government, in April 1893, to pass an Act to obtain State help towards the combating of tuberculosis among cattle. The sum of 100,000 marks, for five years, was apportioned to the Ministry of Agriculture, for the free distribution of tuberculin amongst cattle breeders, for the testing of infected beasts by veterinary surgeons, for circulating instructions as to the necessary means of isolation, and for a general campaign against the disease.

The scheme, briefly, was as follows: An

ordinary herd was taken and every animal tested with tuberculin. The farm was then divided into two portions—a tuberculous division and a non-tuberculous section. Animals reacting went to the tuberculous farm and the remainder to the non-tuberculous enclosure. All animals that were fit for breeding were kept, those obviously unsuitable because of disease were removed. Each herd had a separate staff and the animals were kept entirely apart, even when grazing. Bang then proceeded to breed from them in the ordinary way.

The calf of a tuberculous cow was removed to the non-tuberculous area immediately after birth and fed on the sterilised milk of its parent. The practicability of the plan was soon proved, as the calves which were born in the infected division were practically all found to be healthy, and remained so, provided they were kept free from infection. This system of not destroying tuberculous cows capable of breeding, interfered as little as possible, from the financial point of view, with the farmers, and herein lay its success.

The healthy division was retested twice a year with tuberculin, and any animals reacting were transferred to the tuberculous farm. This number, however, was small. Bang emphasises the fact that the greater was the separation the better were the results.

Bang started his tests in 1892, on the farm of Thurebylille. The herd experimented upon consisted of 208 animals, and of these, 131

reacted, and 77 did not; the latter were young animals. Eighty per cent. was the reaction-rate in milch cows, and 40 per cent. the rate amongst the young cattle and calves. Such results are noticeable throughout his work. The older the animals the greater were the opportunities for exposure to infection, and the higher the percentage of tuberculosis. This herd was divided as described, with separate staff, milking sheds, and fields, and was retested every six months. The newly-born calves were treated on the lines already indicated.

In spite of the fact that isolation was difficult to enforce at first, as the men in charge were not used to the system, the healthy division increased every year, the disease being finally cleared from the farm by selling thirty cattle—all that remained of the reacting section. In 1907, *i.e.*, fifteen years later, not one of the herd, now numbering 211, reacted, although a certain percentage of the parents of the calves were tuberculous until this date. It is clear from such practical experimentation that calves of tuberculous cows, if removed from infection, do not become tuberculous.

The results of two other farms run on Bang's principles, are most interesting. The history of the Borupgaard farm, in Jutland, was as follows:—

The first application of the test was in 1894, on a herd of 225 cattle. Of these, 139 reacted; 82 per cent. were full-grown animals, whilst 86 were found to be healthy. When the test was applied to the herd of 245 animals in 1908, "only one calf

reacted, and so slightly that the reaction must be considered doubtful."

At the Wedellsbourg farm, in Funen, the herd of 240 cattle was first tested in 1896, when 166 animals reacted, and 74 did not. In 1908 there was a stock of 264 beasts, of these only three reacted.

During the period 1898-1904, 40,624 head of bovines were tested in Denmark *for the first time*, with the following results:—

	Reacted, per cent.
Calves under 6 months	12·1
From 6 to 18 „	27·5
„ 1½ „ 2½ years	38·6
„ 2½ „ 5 „	44·9
Those over 5 „	48·0

These were cattle reared on ordinary farms. The statistics show that in spite of the heavy percentage of infection in the cows used for breeding, their progeny started with a very low comparable average — even although exposed to infection from birth onward.

Here, then, is further definite evidence that calves born of tuberculous mothers, and not protected from infection, gradually succumb to tuberculosis as they grow older. That is, the longer the animals are exposed to infection, the more they contract the disease. All this indicates that calves receive a post- and not a pre-natal infection, and Bang[10] is convinced that you can eradicate tuberculosis if you protect the calves from post-natal infection. It is, obviously, not easy to carry out such experiments with human

infants; if it were, there would appear to be little doubt this idea of pre-natal infection would disappear.

The following table, showing the results of the von Pirquet[11] cutaneous test as applied to children, points very strongly to the view that children are born free from tuberculosis.

0 to 3 months	.	0	per cent. of the children tested gave a positive reaction		
3 „ 6 „	.	5	„	„	„
6 „ 12 „	.	16	„	„	„
1 „ 2 years	.	24	„	„	„
2 „ 4 „	.	37	„	„	„
4 „ 6 „	.	53	„	„	„
6 „ 10 „	.	57	„	„	„
10 „ 14 „	.	68	„	„	„
Over 14 „	.	90	„	„	„

Sperk[12] tested 159 infants under three months of age, and none gave a reaction.

Faludi[13] applied the von Pirquet test to 195 infants shortly after birth, and failed to obtain one positive reaction, although 126 of the mothers did so respond. All this is evidence that children do not respond to a tuberculin reaction in the earliest days of their lives; but, like the calves, they respond as they advance in age, and meet with greater opportunities for infection.

Age.	Number examined.	Number found to be tuberculous.	Tuberculous percentage of those examined.
Newly-born .	198	0	0
0 to 1 year .	109	11	10·1
1 „ 5 years .	63	26	41·3
6 „ 15 „ .	26	17	65·4

Beitzhe[14] performed *post-mortem* examinations on infants and young children, with the results as given in table on p. 33.

I have known numbers of children born of ex-tuberculous carrier patients (*i.e.*, with tubercle bacilli in their sputum, for years after their discharge from the sanatorium) and such children were, as far as I could tell, free from tuberculosis. The children of these parents were far above the average in healthy appearance, due to the hygienic conditions their parents had been taught to adopt while at the sanatorium. I would agree with Karl Pearson that there certainly is a soil, or, as he calls it, a diathesis, suitable for the growth of tubercle bacilli in many children born of tuberculous parents. But I would insist, from personal observation, that this soil is due more to the unhealthy environment under which the parents exist than to the presence of the bacillus itself.

V

THAT A TUBERCLE BACILLI POSITIVE EXAMINATION OF THE SPUTUM IS IN ITSELF EVIDENCE OF ACTIVITY OF THE DISEASE

This is a very widely accepted idea, and one of the most misleading. There are several solid facts against the acceptance of such a theory.

A young medical man, who has since become a great friend of mine, was engaged in his routine work as house physician. He felt in his usual

health, but had, however, a little sputum. Out of mere curiosity he stained and examined it. To his great surprise it contained tubercle bacilli. He was, naturally, much upset (although it was an "early case") and consulted the physician of his wards.

The result was that he was sent for two months to an institution *called* a sanatorium, but really a "home for tuberculosis." He was kept in bed, and fed on milk, eggs, butter, etc., and was generally over-fed. He then commenced to feel ill; so he once more consulted his physician, who sent him to Brompton Hospital Sanatorium, where I made his acquaintance. There he went through all the grades successfully, and was discharged as "arrested, with tubercle bacilli still present." He was by then fully convinced which was the better place for treatment, a sanatorium, and not a "home for tuberculosis." He has since then, 1908, remained in perfect health, so far as the tuberculosis is concerned, and able to do his work. He still finds tubercle bacilli when he has any sputum. I quote this case at length, because it demonstrates that a medical man who knows that he has bacilli in his sputum, need not necessarily mean any sort of activity.

Sir James Fowler[15] quotes the case of a patient who was known to have tubercle bacilli in his sputum for twenty years, and yet not to have given any evidence of activity.

I have followed up and kept in touch with ex-patients who have had bacilli in their sputum for several years, and who have been able to

perform their work and have felt in perfectly good health, except for minor ailments, all the time.

There is also the *post-mortem* evidence in the work of both Nageli[16] and Burkhardt.[17] This shows that a large number of persons who, having died of causes other than tuberculosis, presented *post-mortem* proof of that malady. These persons were unaware of their tuberculosis infection in any way; many must have had sputum which, had it been examined, would have given evidence of the existence of the bacilli.

I have known many hundreds of patients who have been discharged from sanatoria as fit for the hardest manual work, but who still have had bacilli present in their sputum. Their classification on discharge has been, "Arrested + tubercle bacilli still present." They have been thus designated because they have satisfied the auto-inoculation test* of performing the hardest possible manual labour for five hours a day without symptoms or rise of temperature, and, usually, with a great diminution in the quantity of their sputum. It is a fair deduction, therefore, to call them "arrested," although, like typhoid carriers, they are capable of infecting others. The typhoid

* Auto-inoculation is the passage of bacteria, or their products, from an infected area into the blood- or lymph-stream. Such a transfer can occur spontaneously or be induced by exercise, active or passive. It results in the stimulation of the defensive mechanisms of the body unless the dose of the bacterial products is excessive. In the former case, the auto-inoculation is beneficial in its effects, and in the latter detrimental, because the natural defences are swamped.

carrier in no way suffers from the disease; just as these tuberculous persons feel completely well, and able to work, although bacilli still persist in their sputum.

If the opsonic indices of these patients were taken, some would undoubtedly show a very high, or varying, index to tuberculosis, as in the Chart 6 on page 54. Although, therefore, the men had such an index and were in perfectly sound health, they could not, strictly speaking, be called inactive, as although their progress was in the right direction, the lesion was open to the blood. Other patients would give a normal index, just as did the man in Chart 6, who, a few weeks previously, had given a positive index. So that in cases of "arrested + tubercle bacilli," some are truly arrested, and some are not. You cannot determine this by the presence of the bacilli, because they are present in both classes. They are, therefore, present when the lesion is open to the blood-stream, and when it is shut off from it, and their presence or absence is no indication as to whether or not auto-inoculation can be induced.

It follows that these persons, though technically, are not clinically active. And yet bacilli are present in these cases of arrest + bacilli; even in those of the truly arrested cases. Many of these ex-patients have proved the permanency of their "arrested" state by their after-histories, even although the bacilli had remained with them for many years.

On the other hand, a patient may have definite tuberculosis, as is indicated by unmistakable,

though few, physical signs, together with fever and definite history. He may also be extremely ill, and yet have no sputum, or if he has any, repeated microscopic examinations give negative evidence. Therefore the presence of tubercle bacilli in sputum does not indicate active disease, nor does their absence permit of a non-tuberculous diagnosis. Persons, therefore, can be carriers without being sufferers from the malady. They may also be carriers before they are patients, or they may be carriers after having been patients, or they may be only carriers. That is, they may be infected and yet never suffer any ill effects from the disease. Thus it is possible for persons to feel in perfect health, and yet to have tubercle bacilli present in their sputum.

How, therefore, can the presence of bacilli prove activity? The value of a positive tubercle bacilli examination is that it *proves infection*, and that is all that should be accepted from this evidence.

VI

THAT A TUBERCLE BACILLI NEGATIVE EXAMINATION OF THE SPUTUM IS EVIDENCE OF NO TUBERCULOSIS

As a general rule, a few negative examinations of the sputum mean nothing. Certainly they do not justify the statement that a person is free from tuberculosis, either active or quiescent.

Norris and Landis[18] state: "In the earliest

stage of tuberculosis the sputum will be found negative for tubercle bacilli in from 60 to 70 per cent. of cases."

Gerhardt[19] writes: "I know cases in which bacilli were not found for months, or only in the sixtieth specimen, but where the diagnosis of tuberculosis was established by examination of the chest."

Eichorst's views[20] are as follows: "Now and then one certainly finds cases in which the tubercle bacilli are not found in the sputum, even after daily examination, and in spite of the undoubted existence of pulmonary consumption."

Turban[21] quotes a case of what proved to be tuberculosis with abundant sputum, free from tubercle bacilli, in which, after a dose of tuberculin and a febrile reaction, tubercle bacilli were demonstrated in the sputum.

Lord[22] says: "It is well known that such negative evidence is of little moment in early cases."

Cabot[78] is of opinion "that no weight should be laid upon the absence of tubercle bacilli in the sputum in this case. Such tests are of importance only when many times repeated. Even then it is by no means conclusive, as bacilli may not appear for many weeks, or even months, after the onset of the disease."

I need hardly add that my own experience agrees with these statements. I have more than once succeeded in finding bacilli on the twenty-first examination, and I recollect an advanced case, with very large cavities and extensive evidence of

lung involvement, in which after twenty-one examinations, tubercle bacilli were not found. There was no doubt about the tuberculosis, as he died from it.

Rutledge and Crouch,[23] reviewing the after-results of the Woodmen Sanatorium, Colorado, U.S.A., give statistics concerning 1654 patients who had been discharged from four to nine years, and make the following significant statement:—

"That the presence or absence of tubercle bacilli in the sputum should be considered in the prognosis. Practically proved cases of tuberculosis with an absence of bacilli in the sputum, even though the lung findings are sometimes much more extensive than those in which the bacillus is present, usually give a much better prognosis."

Klebs,[24] in his book, states that sputum which is microscopically free from tubercle bacilli will infect guinea-pigs. With the help of Dr Distaso I made the following experiment to ascertain some idea of the percentage of such cases. Twenty-one patients were selected whose sputum had been pronounced free from bacilli. These sputa were re-examined until each had given seven negative results. The sputa were then inoculated into guinea-pigs. The animals were killed as they were found to be infected by tuberculosis. Those that gave no evidence of disease were killed at nine weeks, eleven were found to be infected, and ten free.

This does not absolutely prove that the ten sputa were non-infective, for two reasons: (1) because the sputa collected one day may possibly

be free and infective the next; and (2), if the guinea-pigs had been allowed to live longer they might have developed tuberculosis. It does absolutely prove that even repeated microscopic negative examinations of sputum cannot be accepted as evidence of freedom from tuberculosis.

It is a common experience to treat febrile tuberculous patients in bed for weeks with either bacilli-free sputum, or no sputum at all, and two or three months later to find they have sputum containing tubercle bacilli.

There are also the cases of definite tuberculosis, as diagnosed by physical signs in the lungs, by the history, and by the symptoms, but who have no sputum, or sputum which gives negative results. Frequently such cases, undergoing treatment by auto-inoculation, develop a temperature of 100° to 101° F. This is often followed by an increase of sputum containing bacilli, and by the time the patient is ambulant, in about ten days, either the sputum has disappeared or the bacilli can no longer be found. This clearly demonstrates that a patient who has tuberculosis may have sputum which is bacilli-free one week and not the next.

In this connection, a search of the medical records of various institutions will show that it has been the practice to discharge certain patients as non-tuberculous. These patients have usually been admitted because from their histories and symptoms a diagnosis of tuberculosis had been made, and they are sent out on the strength of one, or, in rare instances, three negative examinations of the sputum.

A research into the after-histories of these patients should prove very instructive to anybody who is still of opinion that a negative microscopic examination of the sputum warrants the conclusion that a person is free from tuberculosis.

The Phipp's Institute[25] records the following interesting facts. For the seven years ending 1909, 5895 patients were eventually diagnosed as suffering from pulmonary tuberculosis. Of these patients, 3440, or 58 per cent. had had their sputum examined previous to visiting the institute. Of this number, the sputum had been investigated ONCE only in 3004 cases, or 87·3 per cent. The sputum was tested more than once in 436 instances, or 12·6 per cent.

This is good evidence of the practice of considering one search of the sputum sufficient. It is a fair deduction to make that a state of non-tuberculosis was assumed in the majority of cases where one negative result was obtained. In any event, that would agree with my experience of institutional work, where careful histories are taken, namely, that only one test has been made of the patient's sputum before admission. The futility of such a practice is evident.

It will be useful here to consider what is usually meant by a sputum tubercle negative examination.

The material for examination is, outside special institutions, as a rule obtained in the following way: The patient is given a suitable vessel, with instructions to expectorate into it, seal it, and post it to the address given, usually some laboratory where the sputum is examined for half-a-crown

(that is the pre-war figure). The flaw here is that it may, or may not, be sputum. If a negative result is recorded and placed on the patient's history sheet, it certainly is not entitled to be registered "sputum, bacilli absent." The probable much more accurate entry would be, "mucus and saliva, tubercle bacilli not found." This is the first crucial point in sputum examination. Therefore, before dispatching a specimen for examination the sender should satisfy himself that as far as he can judge, macroscopically, IT IS SPUTUM. It is obviously no use sending spittle in order to find bacilli in sputum, but this is frequently done by the patients, and their fate decided on such an examination.

In order to obtain the best results, the instructions to the patient should be as follows :—

"Cough up into this bottle, the FIRST phlegm that appears to come from your chest, when you FIRST wake up in the morning. One lump is sufficient. Do not try to fill the bottle. But do try to obtain the very FIRST bit of sputum you cough up from your chest, the FIRST thing after you wake." Repetition gets things done.

The examination means everything to the patient; it is essential to take the trouble to endeavour to obtain a specimen of SPUTUM.

To make clear the second point, it is necessary to digress for a moment. At any large institution for tuberculosis it is usual to have a laboratory, and to examine the specimens on the spot. There is thus a direct connection between the patient and the laboratory. The man examining the

sputum knows the patient, and has a clinical interest in him. He has, probably, already made up his mind as to whether or not the person is likely to have bacilli in his sputum, and he searches accordingly. He may keep on looking down at one specimen for over half-an-hour without result. Next day he will have another try. He will be still more particular concerning the actual morsel of sputum he will select to make his film. Extra care will be taken with staining, etc. Bacilli ought to be there, and he means to find them. If he again fails, he tries again. He has become determined that he will not be beaten. So he gives up the ordinary methods, and attempts once more, by concentration and different ways, to prove that after all he was right. In other words, the man's sporting instincts have been roused. If all these researches draw a blank, he will probably inoculate an animal, although this method of settling the matter will not appeal to him very strongly, as he will have to wait several weeks for the answer.

Compare this keen hunt with a purely laboratory worker engaged, week in and week out, with the monotonous routine of daily staining and examining specimens, often of saliva, in order to find tubercle bacilli. There has been very little care bestowed on the collecting of the sample, and he knows it, as he thinks, often aloud, "What's the use of sending this muck to me? it's nothing more than spittle." He is totally unaware of the clinical condition of the patient, and therefore has no personal interest as to the result of his work. As I have indicated, the

longer you look at sputum, the more likely you are to find bacilli. I do not suggest that the chances of seeing bacilli are increased proportionately by the length of time spent on the examination. But that by taking over half-an-hour to examine very many portions of the sputum, it is much more likely to find bacilli than by merely regarding one or two fields. The purely laboratory worker cannot, from his want of clinical knowledge of the individuals, divide the sputa up into those which require a long examination and those that do not, so that he must have a more or less definite period of time fixed for viewing each slide.

A laboratory, with its equipment, material, and salaries for its skilled staff is not a cheap place to run, and that brings us to the question, for how long a time can a skilled man afford to look down a microscope at two-and-six a specimen? And yet on such evidence persons are informed that all is well, because bacilli have not been found in their sputum.

The immense importance of an investigation of sputum surely suggests two things: (1) that the labourer is worthy of his hire (in this case I am taking for granted that the labourer is an expert bacteriologist); and (2) that much more reliable results will be obtained if the laboratory man knows something of the individual who is the patient from his medical attendant. This personal connection will secure good samples of sputum. It is absolutely wrong, therefore, to conclude from even repeated examinations of the sputum, as usually conducted, that—No bacilli means No

consumption! It is essential in all such cases to inoculate a guinea-pig with the suspected sputum. The presence of bacilli in the sputum will definitely prove infection, but not activity. The proof of infection will, however, be of the utmost value in a doubtful case of illness, where there are marked constitutional symptoms and few physical signs.

Not until it is accepted that a negative microscopic examination of the sputum has little if any value, supersedes the present-day, widespread belief that it warrants a statement that a person is not only free from disease but is also not a carrier—not until then shall we see the question of tuberculous infection regarded in its proper perspective.

It naturally follows that "open" and "closed" are insensate and misleading words to use in this connection. From the point of view of the public health authorities, "open" simply indicates that a person is a "carrier." "Carrier" is a word that is so well understood that it is unwise to complicate medical terminology by using the word "open" to represent a carrier. "Closed," on the other hand, denotes too often nothing at all, as a person is so designated after one or perhaps three negative microscopic examinations of so-called sputum.

From the patient's outlook "open" signifies that his lesion is open to his blood, and that therefore he has active disease, whereas "closed" means that the lesion is shut off from the blood, and the disease therefore inactive.

VII

THAT A SUBCUTANEOUS TUBERCULIN REACTION IS EVIDENCE OF ACTIVE TUBERCULOSIS

The reaction in connection with a hypodermic dose of tuberculin is evidence of infection only. Since the temperature is due to the effects of the tuberculin on the tubercle bacilli and their products, it follows that there is potential activity. A person who is apparently quite well may give the reaction, and a patient who remains well many years after treatment may do the same. The reaction taken alone is, therefore, only proof of infection, as the following experiments clearly exemplify.

During the years 1901-2, Franz[26] applied the subcutaneous tuberculin test to 400 recruits who had been examined and passed as fit for the Austro-Hungarian army. These soldiers came from Bosnia, where tuberculosis was prevalent. Sixty-one per cent. (or 245) gave a positive reaction, which, as has just been stated, indicates that they had been infected by tuberculosis.

At the end of a year, he once more tested 100 of these same men. All those who had reacted on the former occasion, responded a second time. Fourteen soldiers who had not previously reacted, now did react. These men had probably become infected between the test doses. A total positive of 76 per cent., as against 61 per cent.

The following year, Franz tested another

group of 321 soldiers from the same regiment; 222, or 68·8 per cent., of these reacted. In order to verify these remarkable statistics, he obtained 279 men from a Hungarian regiment, which was recruited from districts where there was less tuberculosis. In the Bosnian regiment, the total invalidism of 60 per cent. of the men was due to tuberculosis, whereas the Hungarians, during the same period, suffered only incapacity from tuberculosis to 24·8 per cent. Of the total of 279 Hungarians, 108, or 38·7 per cent., reacted. All the men experimented on, including those who reacted, were apparently healthy, vigorous and young—that is, they were under the age of 23.

The test in tabular form is as follows:—

Year.	Number tested.	Reacted.	Per cent.	Tuberculous incapacity to total regimental incapacity.
Bosnians				
1901-1902	400	245	61	60 per cent.
1902-1903	321	222	68·8	„ „ „
Hungarians				
1902-1903	279	108	38·7	24·8 „ „

This demonstrates clearly, that of the Hungarians fewer react, because there is less tuberculosis amongst the troops, as is shown by the lower incapacity.

Of these 1000 soldiers, tested with tuberculin, 46 exhibited definite clinical signs of disease, and four years after discharge, *i.e.*, seven years from reaction, 18 additional men showed clinical evidence of tuberculosis. Of these 64 men, the first test

had been positive in 46, and negative in 18. The percentage amongst the Bosnians was 7·6, and that of the Hungarians only 3·20.

These figures are thus summarised :—

Number tested.	Number reacted.	Per cent.	Number subsequently proved tuberculous.	Per cent. of total tested.	Per cent. of total reacting.
1000	575	57·5	64	6·4	11·11

It is manifest from these details that more than half the healthy soldiers had been infected, and therefore reacted. During a period of seven years, only 11·11 per cent. of those who gave a positive tuberculin reaction subsequently suffered from tuberculosis. The probable reason why 18 men did not respond to the first test, is that they then had not a lodgment of infection in their bodies.

This work clearly illustrates that a man may be infected by tuberculosis, and therefore respond to tuberculin, without in any way suffering from the disease.

Should the tuberculin reaction be accompanied by increased physical signs, *plus* sputum, or an increase of, and also, perhaps, the appearance of tubercle bacilli, it is unmistakable evidence that there is *potential activity*, and that this activity can, in all probability, be influenced either by subcutaneous or auto-inoculation. This is called a focal reaction. It is in such a case as this that the dosage of tuberculin requires both knowledge

and experience; because it has been used without either it has earned the reputation of being dangerous. It is no more dangerous than is arsenic in the hands of persons without knowledge and experience in its use.

Some very advanced and definite cases of tuberculosis do not give a reaction; except, therefore, in such a case a negative tuberculin reaction may be taken as proof that the suspected lesion is not tuberculous, and the test here, unlike that of sputum, is very valuable.

Finally, a tuberculin reaction, unless it be a focal one, does not indicate active disease. All the troops were capable of performing their ordinary duties, and were not affected in any way, or they would not have passed the exercise tests (which will be referred to later) imposed on them by the ordinary routine life of a soldier.

VIII

THAT THE VON PIRQUET TEST IS A PROOF OF ACTIVITY

This is a tuberculin test, and as ordinarily carried out, is indicative of infectivity, but not of activity. It does not produce a focal reaction, nor does it demonstrate the site of the lesion. It is an extremely useful test in the case of children, as, here again, a negative reaction will warrant a verdict of no tuberculosis. It has also

the merit of not having the same "frightfulness" to the parents as the word "inoculation."

See von Pirquet's table on page 33.

IX

THAT THE OPSONIC INDEX IS OF NO USE

This test in tuberculosis is proof of infection, and in addition is a test that, under certain conditions, gives evidence of activity. Should these conditions not be observed, it may furnish no clue to tuberculosis, although the patient is actually tuberculous.

It is well known that the opsonic index varies with the bacterial products present in the blood. If a sample of the blood of a tuberculous patient be taken when at rest, and such specimen be compared with another one taken after a six miles' walk, it may be found that the exercise has caused bacterial products to enter the blood-stream. In such a case, the opsonic content of the second sample will differ from that of the first blood taken.

Such a reaction indicates two things:—

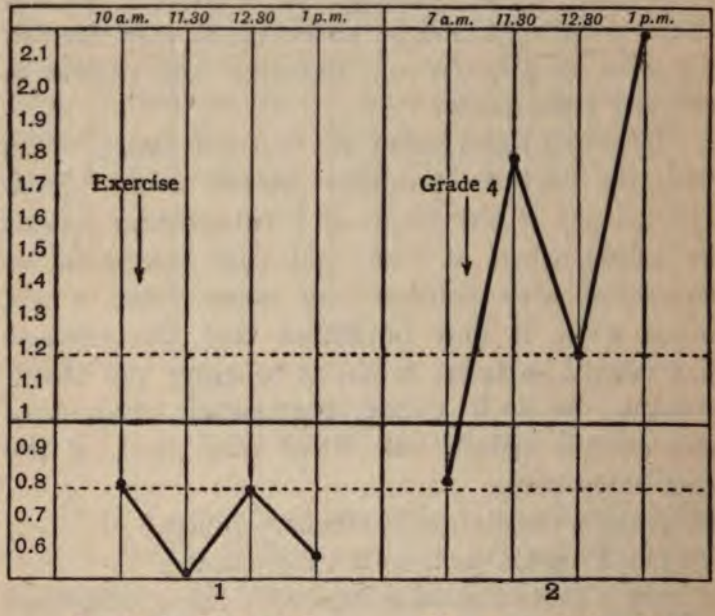
(1) That the patient is tuberculous;

(2) That the lesion is capable of being influenced by exertion, that is, by auto-inoculation, and consequently is potentially active.

The two samples of blood, however, may not demonstrate any difference in the opsonic index.

Does this justify a verdict that the patient is free from active tuberculosis? Most decidedly not. The reason is, that the effort of walking six miles may not have been sufficient exertion to affect the lesion and cause bacterial products to be given forth. It follows, therefore, that a patient cannot be pronounced free from active tuberculosis by this test until he or she has performed the hardest manual exercise of which they are physically capable with a normal opsonic index.

The following Charts demonstrate these facts.

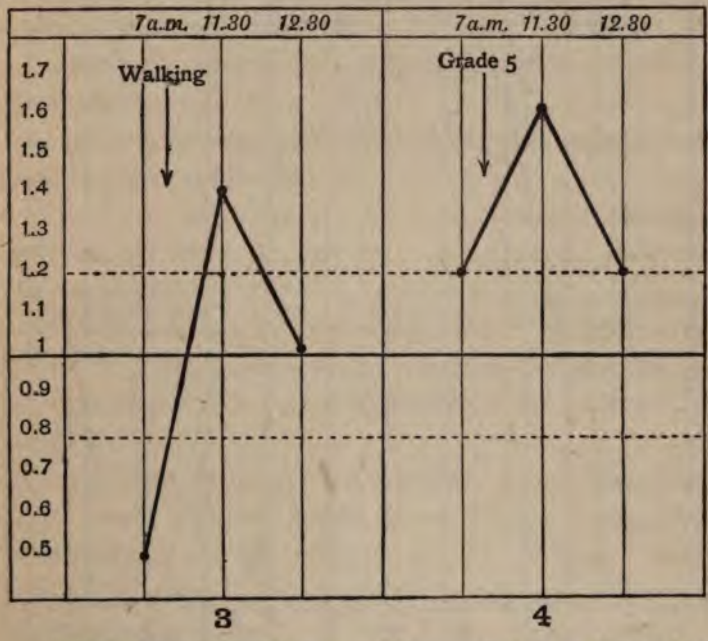


Exercise hardly affects the index.

The same patient on fairly hard work.

In the first, the walking exercise has not been sufficient to influence the bacterial products and

the index has not varied. In the second the patient is on harder work, and the opsonic index indicates that auto-inoculation is taking place. The next Charts, Nos. 3 and 4, show the difference in the

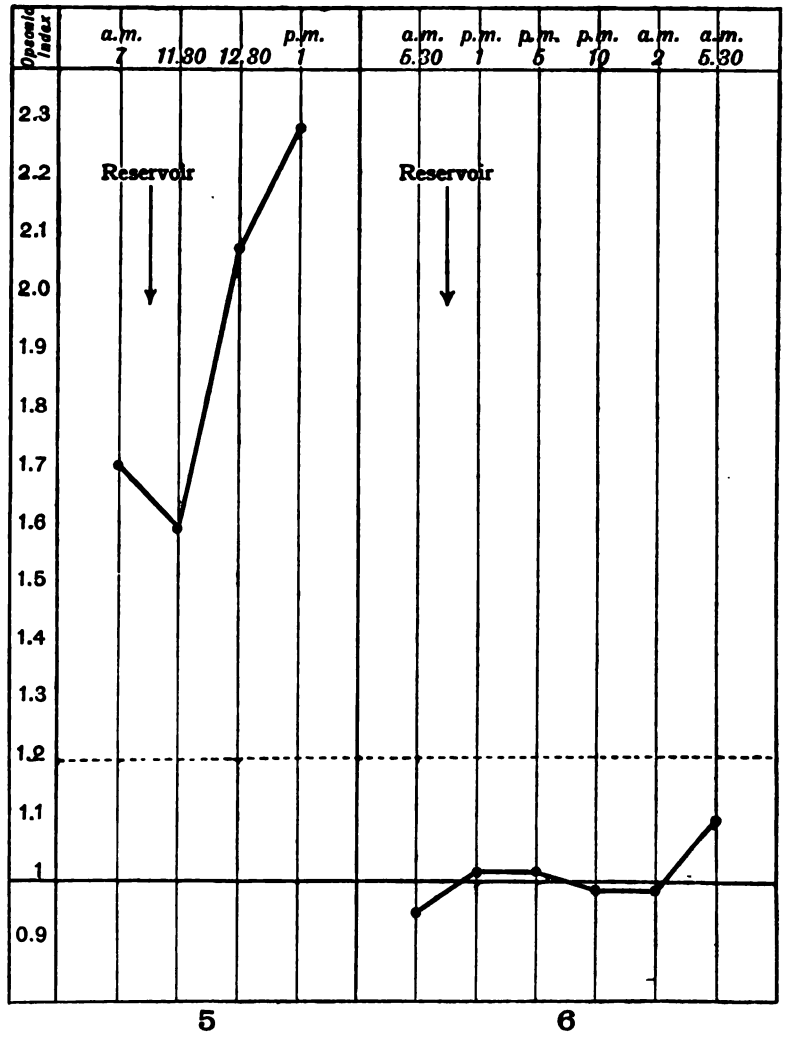


Walking does not produce an adequate response.

The same patient on the hardest manual work.

index of a patient when on walking exercise, and later when performing the hardest work. Chart 5 shows the elevation of the index due to the hardest work, whilst in No. 6 the index is normal, although the same physical efforts have been put forth, *i.e.*, arrested disease.

Hitherto, in connection with diagnosis and activity, my criticism has been chiefly destructive. One may readily ask, how then can you ascertain



Index of patient working as a navvy in the construction of a reservoir.

Index of the same patient, now within normal limits, taken three weeks later under exactly similar conditions of labour.

whether a given person has active tuberculosis and requires treatment, or has quiescent tuberculosis and does not require treatment, apart from the opsonic test, which, from the account that has been given, may take a long period of time?

Briefly: proof of infection can be thus obtained:—

(1) The presence of tubercle bacilli in sputum found microscopically;

(2) The establishing of tuberculosis through sputum, blood, or serum inoculation into a guinea-pig;

(3) Definite, physical signs, and if there is any doubt as to syphilis, as to whether or not these are due to syphilis, a Wassermann test will settle the diagnosis.

Activity.—The opsonic index, blood inoculations and pleural fluids inoculations are the laboratory tests of activity in an afebrile case. Fever in a case of tuberculosis is clinical evidence of active tuberculosis. There is not, unfortunately, at present any rapid, simple, reliable and accurate test for determining whether an afebrile patient's lesion is active. Meaning by active disease, that there is a lesion caused by tuberculosis, which is in connection with the blood fluids, and which can be influenced by rest or exercise. As there is no such test, each case must be dealt with individually, at the time of the diagnosis of the disease. It is not sufficient simply to say a patient has tuberculosis: that only means infection. The patient may require treatment or he may not.

It is not adequately realised that a patient can have active and progressive disease without a rise of temperature. A man's resistance may be just sufficient to counteract the attacks of the bacilli. The day, however, may come when his resistance commences to break, and then the symptoms enumerated below will indicate that the disease is active. Bacterial products are passing into his blood in quantities that are affecting his general health, but not to a degree to cause invalidism.

A simple test that would, in these cases, diagnose the condition we suspect to exist, namely, tuberculosis with activity, would make the tuberculosis problem simple. These are the "early cases," but few at this stage are fortunate enough to be diagnosed.

The chief symptoms indicating activity are as follows:—

Slackness;

A feeling of ill-health;

Feeling done up after a day's work, which formerly was easily performed; especially is this so after "influenza" or "pleurisy";

Loss of appetite, especially when exercise is increased;

Continuous loss of weight in young persons;

Increase of sputum after exercise;

Headache after exercise;

Slight rise of temperature after exercise—99° F. men, 99·6° women;

Marked deviation between morning and evening temperature, and rapid pulse and blue lips, where there is no heart disease.

Every one of these slight symptoms, especially after pneumonia, influenza, pleurisy, or slight hæmorrhage "from the throat," or associated with slight cough with or without sputum, should suggest the most thorough investigation into the question of tuberculosis.

Finally, in searching for tuberculosis, a record of temperature should be regularly taken, a rise to 99° in men, or 99·6° in women, and a headache after exercise, are to my mind diagnostic of activity of tuberculosis, if infection is known to exist. And in any case it is evidence that probably some microbe is at work, if not the tubercle bacillus.

The difficulties of deciding as to whether a person, definitely infected with tuberculosis, and who appears to be in good health, has or has not potentially active disease, must be apparent, and yet many such persons are advised to go to Switzerland or to the seaside without any real attempts being made to ascertain this crucial point. If the disease is inactive, there is no purpose in advising treatment. If the disease is active, climate is not treatment, as will be demonstrated later on.

X

THAT A BLOOD INOCULATION IS USELESS

This is a test which, if positive, indicates both activity and infection at the same time, namely, the inoculation of blood into a guinea-pig. After a certain period the pig is killed, and if found to be tuberculous, it proves that the blood was infectious, and therefore the disease active. On the other hand, a negative blood inoculation test neither means that the patient is free from infection nor that he has not potential activity.

And this for two reasons: (1) the bacilli may not have been in the patient's blood when the sample was taken; and (2), it may take longer than two months for tuberculosis to develop in a guinea-pig. This is the usual time at which a guinea-pig is killed, if it has not previously given evidence of sickness.

The rôle of the tubercle bacilli in the blood is not yet fully determined. It is known that blood from patients with tubercle bacilli in their sputum, inoculated into animals, has not produced tuberculosis.[43] It is equally certain that similar blood has produced tuberculosis in the pigs. Liebermeister[27] found in the bloods of patients who had advanced disease, and who would be classified by Turban as stage 3, that 60 per cent. of the animals showed evidence of infection. The percentage was much lower in the second stage, and none of the guinea-pigs was infected by the

bloods drawn from the patients ranged under class one.

It occurred to me, several years ago, that the reasons why there were these differences, were associated with the time when the blood specimens were taken, the time depending on whether or not there had recently taken place a passage of bacterial products from the lesion to the blood-stream. If the lesion were inactive, it would not be reasonable to expect to find bacilli in the blood. On the other hand, if these bacilli were passing into the blood-stream, there should be a possibility of finding them after an auto-inoculation.

When a patient suffers from continuous fever due to tuberculosis, it is an indication that he is receiving excessive auto-inoculations of his own bacterial products. If tubercle bacilli are ever to be found in the blood, with a reasonable amount of certainty, it is at such a time they ought to be demonstrable, but the blood must be taken soon after the auto-inoculation.

The more advanced the case, and therefore the more frequent the auto-inoculation, the more likely it is to find the bacilli. In such cases, the difficulty, unless a continuous temperature record be taken, is to know when the auto-inoculation has taken place, as experiments have shown that bacilli injected into the blood-stream of animals rapidly pass out of the general circulation.

It is probable that blood specimens withdrawn from febrile tuberculous patients, haphazardly, so far as the times of auto-inoculation were concerned, would give the variable results described by

Liebermeister. Which means, that by chance some specimens would be taken near to an auto-inoculation and would give a positive result, whilst others would not, and therefore show negative results. The auto-inoculations would be less frequent in the less advanced cases, and so the number of animals infected would be fewer.

It is necessary to recall that patients undergoing treatment by auto-inoculation quite commonly receive a rather larger dose than usual of their bacterial products, and consequently suffer from sudden onset of fever, from 100° to 101° F.

Here, the time of the auto-inoculation is definitely known, and therefore if the blood sample be taken at that time, tuberculous infection should be conveyed to animals. If this theory is correct, bloods taken after the temperature was normal, and the patient ambulant again, should not infect guinea-pigs.

During the war I took a number of blood samples from this point of view, but owing to difficulties entailed by not having a licence and the scarcity of guinea-pigs, only four of the febrile bloods were examined in this manner. All four were cases of indefinite disease, and I was using the method for diagnosis, as well as for proof of tubercle bacilli in the blood. Two of the febrile bloods conveyed tuberculosis to the guinea-pigs, and the blood taken ten days later from the same two patients, when their temperature had been normal for seven days, did not infect the guinea-pigs. The other two bloods, from febrile patients, did not infect the pigs, and so no second

samples were taken. These two guinea-pigs were killed at the end of nine weeks. If they had been kept for longer periods they might possibly have shown evidence of tuberculosis, so that it is not absolutely certain that all the bloods were not tuberculous.

It is already known that tuberculous infection can be obtained from the blood of patients who may or may not have other evidence of infection; therefore the only deductions to be drawn are, that if auto-inoculations be properly controlled the presence of tubercle bacilli in the blood is not harmful to the patient, and that the matter requires further research. If the idea is correct, the test will be of great value in early diagnosis of tuberculosis. It has already been affirmed that many a case of tuberculosis starts its career under the designation of influenza or pleurisy.

If, therefore, the bloods of such patients are taken at the onset of the symptoms, a diagnosis may be established at the outset by this very simple animal experiment.

A positive result would be of enormous value to the patient, whose treatment could be entered on immediately, instead of waiting for a further and perhaps more serious manifestation of the disease, with therefore a more remote chance of recovery.

In reading through the literature of tubercle bacilli in the blood, one is struck by the fairly unanimous opinion that no help can be had at present from specimens of blood examined microscopically.

XI

THAT CLUBBING OF THE FINGERS IS A SIGN OF TUBERCULOSIS

It is very uncommon to find clubbing of the fingers in tuberculosis, except in the later stages of the disease, and even then it is usually absent. It is, however, almost diagnostic of bronchiectasis, especially if it is well marked. Amongst 3557 cases of tuberculosis seen at Phipp's Institute, twenty-eight varying degrees of clubbing were noted in 772 cases examined, *i.e.*, 21·7 per cent. Fifty per cent. of these cases were from two to five years old when first seen, and 21 per cent. had suffered for five years before presenting themselves to the institute. It should not be overlooked also, that tuberculosis and bronchiectasis may co-exist. The number of cases of really marked clubbing, occurring in recently infected persons suffering from tuberculosis, is so very small as to be of no help in diagnosis. So that clubbing, especially if there is a history of large (a pint or over) and repeated hæmoptysis, should suggest bronchiectasis rather than tuberculosis.

XII

THAT TUBERCULOSIS CAN BE DIVIDED INTO THE THREE DISTINCT FORMS—ACUTE, SUBACUTE, AND CHRONIC

Leaving miliary tuberculosis out of consideration, pulmonary tuberculosis cannot be subdivided into three such water-tight compartments. It can only be arbitrarily divided into two forms, "active" or "inactive." Either there is a lesion open to the blood-stream, or there is not. In the one case, therefore, it is active, in the other it is not.

Auto-inoculation in pulmonary tuberculosis occurs in at least three ways:—

(1) Auto-inoculations occur and invocate immunising responses, without in any way affecting the general health or feelings of the person infected. Good examples are Nageli's cases,[16] who gave evidence of healed tuberculosis, but no history of illness.

(2) Those patients in whom the auto-inoculations produce slight symptoms and, at times, deviations in the temperature below normal.

(3) Those patients in whom these produce marked constitutional disturbances, accompanied by fever.

All three, therefore, indicate active disease. The first class, obviously, would not seek treatment. So that in discussing active disease, for the sake of simplicity I shall leave that class out of consideration, and consider only classes two and

three, where symptoms, etc., indicate auto-inoculations are taking place. The Charts here given (pp. 66-70), showing temperature of patients who have undergone auto-inoculation treatment, will indicate what I mean.

Chart 7 demonstrates the case of a patient with active disease. That he is having constant small responses can be seen from the deviations in the temperature. There is, however, no rise of temperature above normal. In the last line of the Chart, the deviations disappear as the responses cease on the hardest grade of physical effort. These active but non-febrile cases, according to the nomenclature given above, would be called chronic, some persons even call them convalescents. This type of case is usually classed as not an active one, because of the absence of fever. I have frequently been informed that such cases regain their health without treatment. The exponents of this view would prove their ideas mistaken, if they were to start their patient on grade 6 instead of grade 1. They would find him very active from the temperature standpoint next day. These patients, therefore, are not in any way necessarily chronic merely because their temperature does not rise above normal. Obviously, some are active, and some are not. The only way to find out whether or not there is activity, is to test them by graduated exercises. This will decide whether a patient has arrested disease, or whether he is to be classed as a chronic invalid.

The word "convalescent" cannot strictly be applied to tuberculosis. It means to recover

strength AFTER an illness. If the tuberculosis is past, how can a person suffer from it?

Chart 8 illustrates a patient who has active disease, and a focus that is easily influenced by exercise. For want of a better title, I call these "sensitive" cases. Auto-inoculations are easily induced, and as easily controlled. The patient eventually reaches the pick-axe stage, and as his immunity rises so does his temperature regain the normal line, and the deviations disappear. This would be called the subacute form. The subacute form really indicates that the patient is sensitive, and is walking about without method in the control of his auto-inoculations. If such a sensitive man remains in a bath-chair, or in bed, on complete immobilisation his temperature will be normal. Rest, therefore, alters his classification from subacute to chronic. By permitting him to become "chronic" in the bath-chair, he is losing his opportunities for gaining immunity by his own auto-inoculation.

In such a case, the temperature depends not on whether the disease is chronic or subacute, but on whether or not his auto-inoculations are effectively controlled. Hence the difference between chronic and subacute tuberculosis depends on the control of the auto-inoculations. "Subacute" cannot therefore logically be called a form of disease, it is only a passing event.

Charts 9, 10 and 11 illustrate the cases of three patients undergoing treatment which caused auto-inoculations sufficient to create constitutional disturbances. This toxæmia was controlled by

CHART 7.—Showing deviations of the temperature throughout the grades. Finally, the patient performs the hardest work without such deviations.

CHART 8.—Indicating a patient very susceptible to his bacterial products, large and frequent auto-inoculations being controlled by immobilisation. Finally, the patient works as a navvy for 5½ hours daily with an even temperature. At the commencement of treatment he had about 2 ounces of sputum per diem. This disappeared on grade 4, returned, and again disappeared on grade 5, to return and again depart on grade 6.

Chart 7.

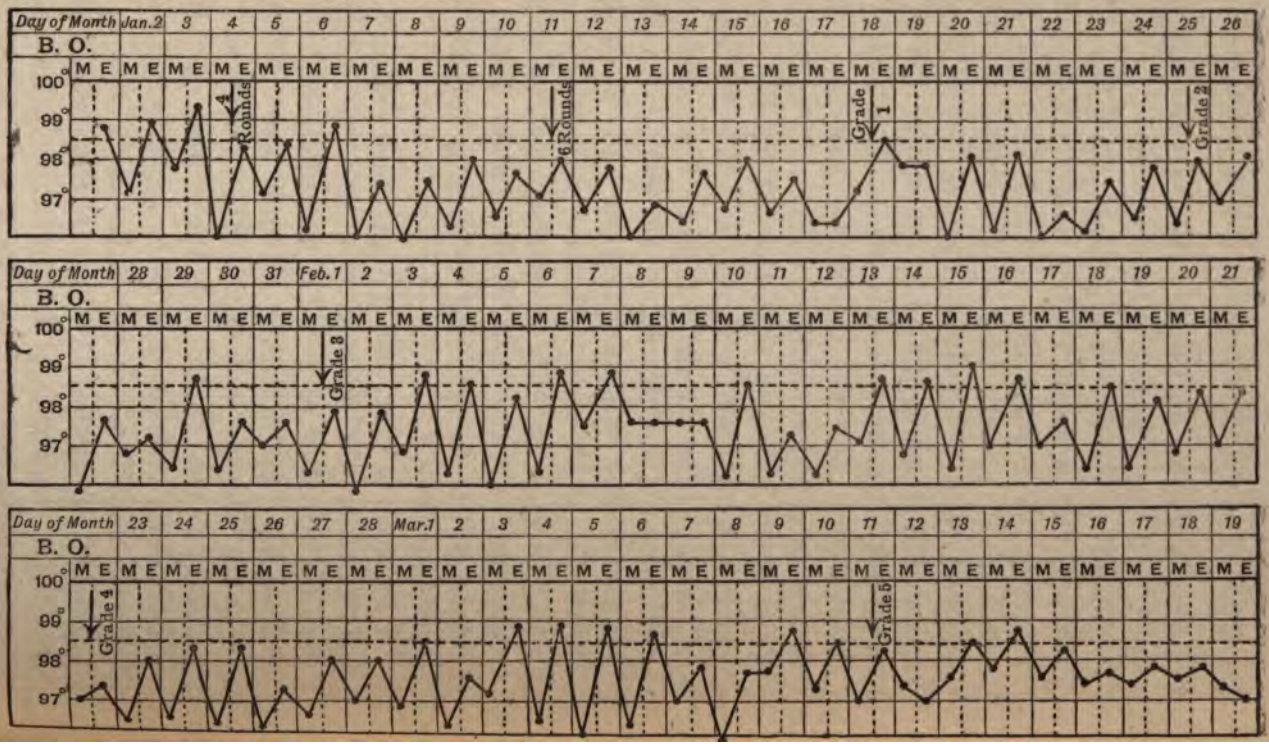


Chart 7.

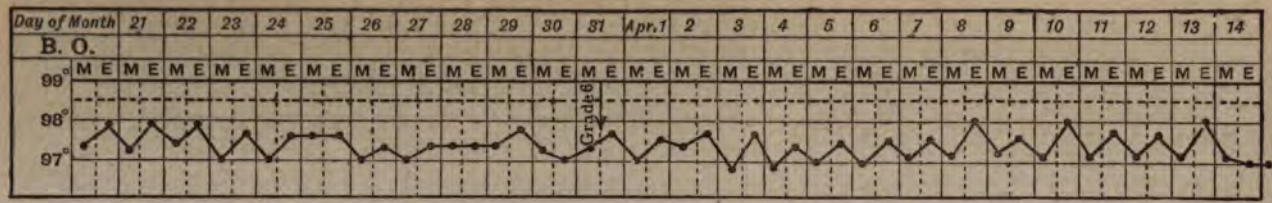


Chart 8.

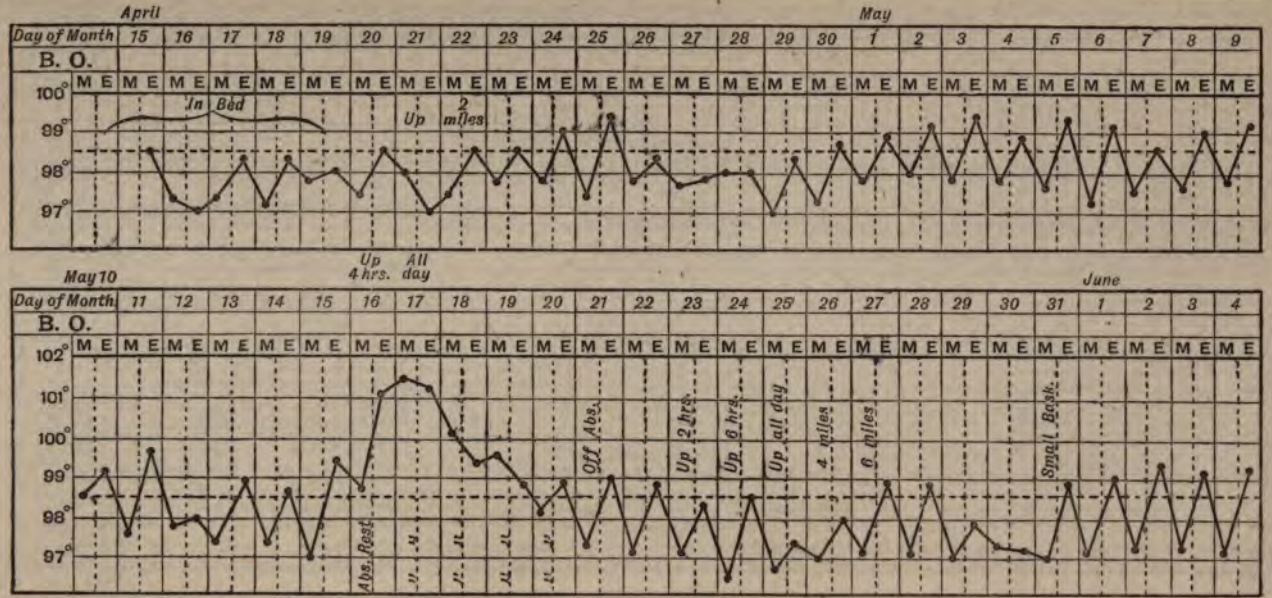


Chart 8.

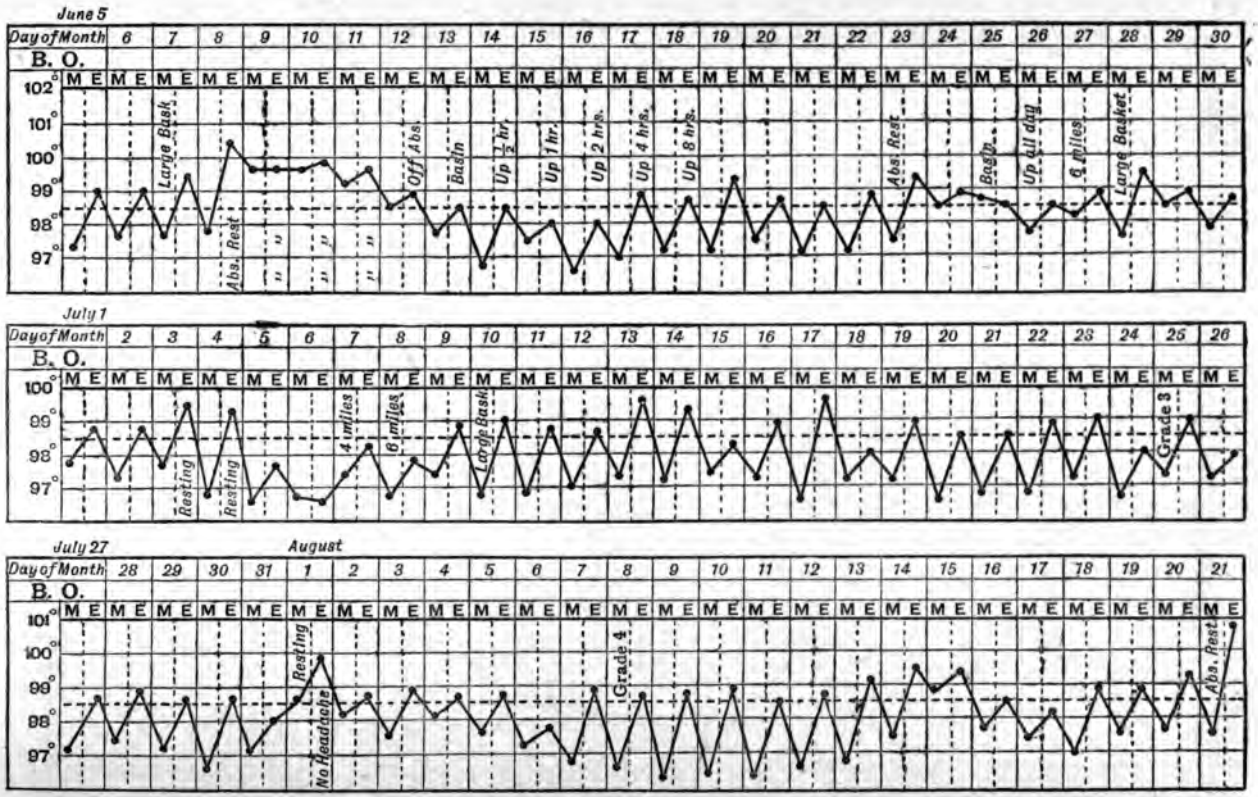
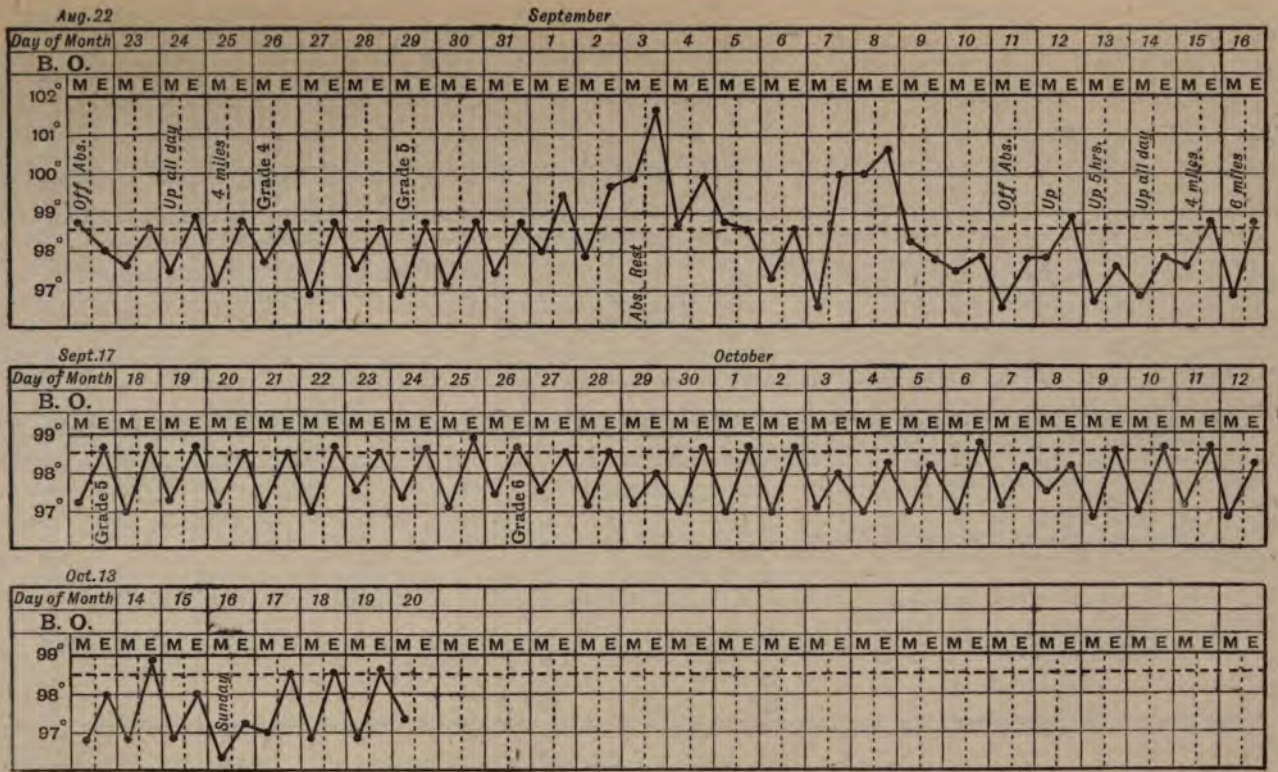

June 5
Day of Month 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30
B. O.
M E
102°
101°
100°
99°
98°
97°
Large Bask.
Abs. Rest
Off Abs.
Basin
Up ½ hr
Up 1 hr.
Up 2 hrs.
Up 4 hrs.
Up 8 hrs.
Abs. Rest
Basin
Up all day
6 miles
Large Basket
July 1
Day of Month 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26
Resting
Resting
4 miles
6 miles
Large Bask.
Grade 3
July 27
August
Day of Month 28 29 30 31 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21
Resting
No Headache
Grade 4
Abs. Rest

Chart 8.


Aug. 22
September
Day of Month 23 24 25 26 27 28 29 30 31 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16
B. O.
M E
102°
101°
100°
99°
98°
97°
Off Abs.
Up all day
4 miles
Grade 4
Grade 5
Abst. Rest
Off Abs.
Up
Up 5 hrs.
Up all day
4 miles
6 miles
Sept. 17
October
Day of Month 18 19 20 21 22 23 24 25 26 27 28 29 30 1 2 3 4 5 6 7 8 9 10 11 12
99°
98°
97°
Grade 5
Grade 6
Oct. 13
Day of Month 14 15 16 17 18 19 20
Sunday

Charts 9, 10, 11.—Temperature Charts of three patients who had large auto-inoculations, which were controlled by complete immobilisation, so that they were able to return to the same work which caused the fever within ten days.

Chart 9.

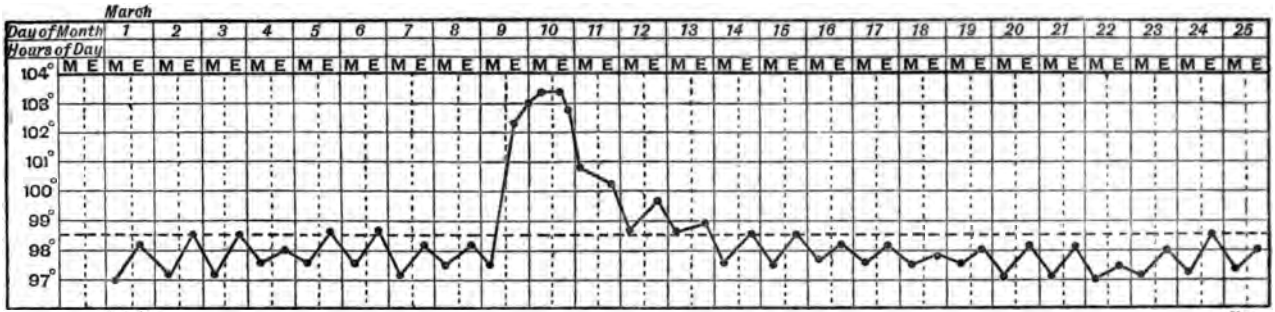


Chart 10.

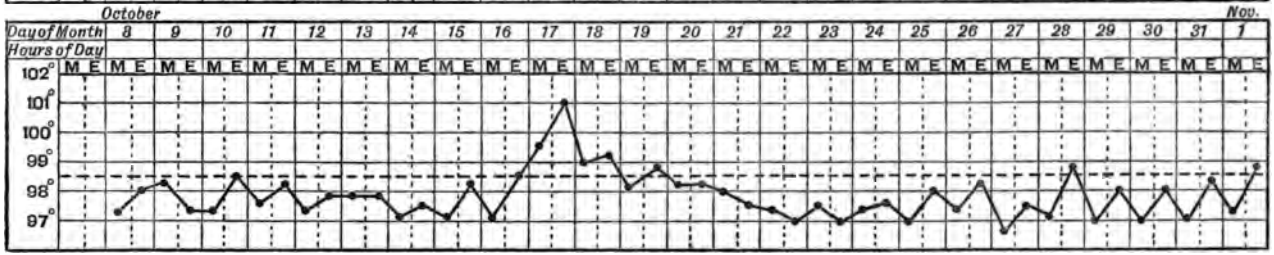
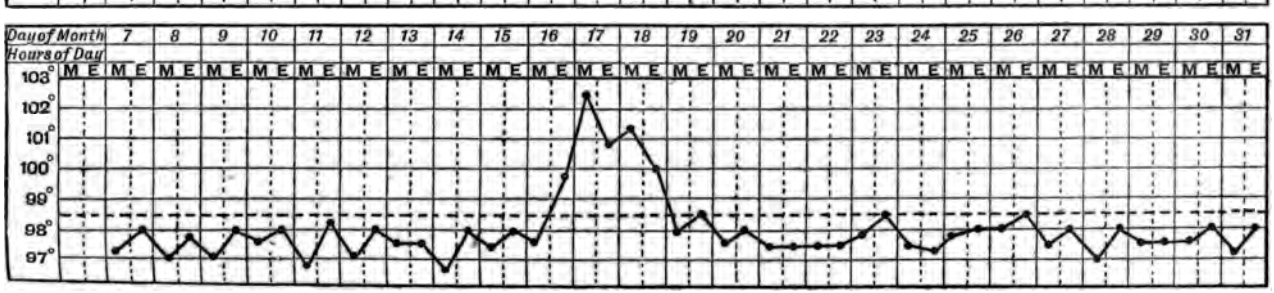


Chart 11.



immobilisation, with the result that the patients went back in ten days to the work that occasioned the toxæmia and the rise in temperature.

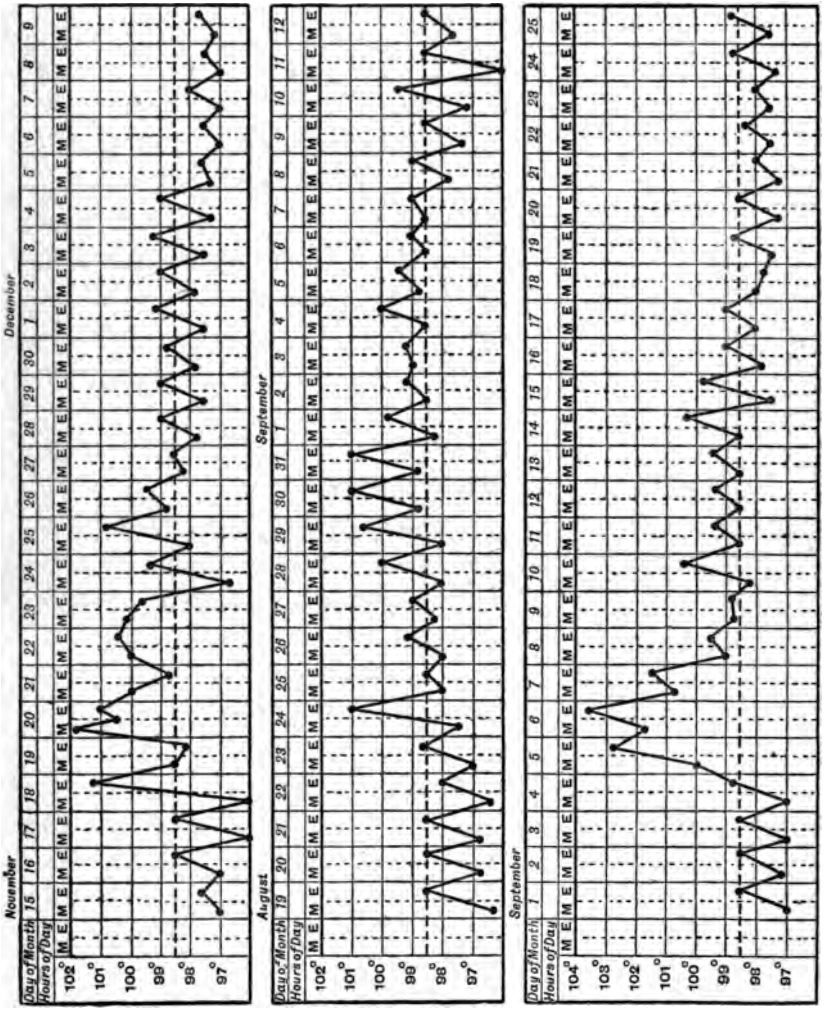
Charts 12, 13 and 14 represent three other patients who, whilst undergoing similar treatment, also had large auto-inoculations. In these instances the inoculations were not controlled by immobilisation, and the temperature did not return to normal in so short a time. These six cases should all, according to the terminology under consideration, be designated acute. They would not, however, at that stage of illness have been acute unless their auto-inoculation had been induced.

Thus by properly controlled auto-inoculation it is possible to turn a "chronic" case into an acute one, and by proper rest to change it back in a few days into a chronic one. Even although the case has been acute, it is possible for the patient to return to the work that caused him to be "acute," and not become "acute" on it again, for the reason that immunising responses have followed the auto-inoculation.

The use of the terms Acute, Subacute, Chronic, certainly suggests that there are three forms of the disease, whereas, in fact, there is only the one without the prefix. These various phases merge one into the other, according to the induction and control of the patient's bacterial products. No other blood disease is subdivided into different forms because of the fever.

For the past five years I have been the "host" of the *Bacillus melitensis*, and during that period I learned something of his activities. With a normal

CHARTS 12, 13, 14.—Temperature Charts of three patients on work which caused large auto-inoculations, but who were not treated on the lines of complete immobilisation.



temperature he can discharge his products in sufficient volume to make one disinclined to work. He also is able actively to invade the knee-joints, and that without fever. He may, should he be so disposed, administer a sufficient dose of toxins to render it necessary to seek repose for several days. When his inoculations are finished, one often feels the better, and certainly able to return to work. The *Bacillus melitensis* is not unlike bacillus of tuberculosis. He remains a long time with his host, and by his activities or otherwise, causes or does not cause auto-inoculation of his products.

Malta fever has not been divided into three distinct forms, although it so closely resembles tuberculosis. It has been called "undulatory" fever, because with the auto-inoculation the temperature is sometimes up and sometimes down, and if there must be some other classification for this disease, when active, other than pulmonary tuberculosis, "undulatory" would certainly be a much more enlightening word.

XIII

"ONCE TUBERCULOUS, ALWAYS TUBERCULOUS"

This saying would be plainer if it were written, "once a patient has had pulmonary tuberculosis, he is never allowed to suffer from any other disease." I can best illustrate my meaning by describing the case of a particular patient.

The patient broke down from pulmonary tuberculosis shortly after the birth of her first child. The disease spread so rapidly that within two months four lobes were involved, including the whole of the left upper lobe. After a long period of complete immobilisation she went through a course of auto-inoculation, the hardest exercise being in the form of riding really hard for five hours a day. After satisfying the final tests, she still had many crepitations over the left upper lobe. There was, however, no sputum and no evidence of activity in any form.

After treatment she used to move about a great deal, and every time she had a slight temperature, fresh medical attendants, of necessity, were called in to attend her. They naturally would take the temperature, examine the chest, and hear the crepitations. She was then always informed that she had active tuberculosis once again, and that she must go to bed for a time. I then had to settle what was wrong with her. If she was far away, her medical adviser corresponded, but if near at hand, I would see him. On each of eight occasions, in twelve years, I pronounced against the tuberculosis, and as luck would have it, I proved to be right, because I knew her physical signs and her general condition with regard to auto-inoculation. But without this knowledge it would have been almost impossible to have come to such a conclusion.

I have narrated this case in detail to make clear my meaning; it is, however, by no means an exception. Very many ex-sanatorium patients

have written to me concerning their similar experiences, namely, that any rise of temperature is nearly always believed to be due to tuberculosis rather than to any other disease.

These facts, therefore, should never be overlooked :—

(1) That a patient with fever and crepitations in the lung, is not necessarily suffering from tuberculosis ;

(2) That crepitations remain for many years, where a lesion has healed, and do not mean active tuberculosis—another indication that the stethoscope has its limitation—and clear evidence of the clinical difficulties in the differentiation between the various causes of fever.

XIV

THAT KNOWLEDGE OF THE ACTIVITY OF THE DISEASE CAN BE ASCERTAINED FROM PHYSICAL SIGNS ALONE

During my student and early days of medical practice, when tuberculosis was included in the diseases of the lungs, in all text-books and lectures, great stress was laid upon the importance of physical signs in tuberculosis, both as to the extent of the disease and the presence or absence of cavities. Attention was, in those days, rightly centred on the physical signs, because it was considered to be a lung disease. Now we know

that tuberculosis is a general disease, and that the physical signs have not the importance that was formerly attached to them. Therefore it is necessary to look elsewhere than in the lungs to obtain information as to the general health and progress of the patient.

To make this clear, let me give an illustration. A patient may have the signs that the whole of the left upper lobe has been rendered useless by tuberculosis. If, however, he is able to perform, for six hours per day, the hardest manual labour without ill effect, then it is apparent that the diseased lung is not adversely affecting his health, and is inactive. Tuberculosis is, therefore, a general disease of the blood fluids, with a local manifestation in the lung. Of chief importance to the patient is his resistance to the bacilli and their products. One man may have entirely lost his resistance to tuberculosis, as is evidenced by the presence of fever and bacilli in the sputum, and yet no physical signs can be demonstrated, while another may have physical signs, involving several lobes with cavities, and yet be able to follow his ordinary occupation.

A carbuncle is, like tuberculosis, a general disease with a local manifestation. In the case of a carbuncle, the local condition has the enormous advantage of being actually observable, and yet with this immense assistance it is not possible to determine the effect of the toxins in the patient's blood by such an inspection. The carbuncle itself may be very painful, but it is not that which affects the general health of the individual but

the poisons produced at the seat of activity of the microbes. Just as it is impossible to observe whether or not the bacterial products of the carbuncle are or are not passing into the blood-stream, so it is equally impossible to make such an observation in a case of pulmonary tuberculosis.

During 1910 I wrote that it was not possible to tell whether or not tuberculosis of the lung was in an active state by auscultation alone, and that the only way to assess a patient's resistance was to test him by graded work. It is, therefore, very interesting to read the following, taken from the Report of the Medical Research Committee on Disordered Action of the Heart.—D.A.H.[29]

"Auscultation is the least valuable method employed in sorting soldiers suffering from cardio-vascular derangement; the sorting can almost always be effected without its aid.

"Certain physical signs, commonly used in sorting these soldiers into the several categories of fitness and unfitness for duty, have received thorough investigation. It has been shown that systolic murmurs, irrespective of their character, point of maximal audibility, conduction, etc., give only a precarious basis for forming an opinion as to future fitness. The upward or downward extension of the apex beat—even to several rib spaces—is also an untrustworthy guide in the same sense.

"In judging unfitness, reliance may be placed on a history of the onset of symptoms due to rheumatic fever upon objective distress, on test exercises upon rapid heart action, with slow decline of rate after exercise, and upon hyper-algesia of the chest-wall. Nevertheless, while all

these give valuable criterions, the soundest method is unquestionably the use of a system of graded exercises. THAT A MAN'S OBSERVED CAPACITY TO ACCOMPLISH WORK OF A GIVEN ORDER IS THE ONLY DEPENDABLE TEST OF SUCH CAPACITY WOULD SEEM SELF-EVIDENT; YET IT IS THE EXPERIENCE THAT MEDICAL OFFICERS RELY MORE UPON PHYSICAL SIGNS OBTAINED WHILE THE SUBJECT IS AT REST."

These statements, and particularly the last paragraph, apply equally well to tuberculosis. It is not the physical signs that matter, but the test whether the patient's bacterial products are or are not under control.

I do not desire to underestimate the use of the stethoscope, but to indicate its proper value. It is useful:—

(1) In making a diagnosis in pulmonary tuberculosis;

(2) In giving evidence of the extent of the disease.

Physical signs are of great value in coming to a diagnosis and help in prognosis, but in treatment they have hardly any importance.

A comparison of the general fitness or otherwise of the patient with the extent of the disease will give a sound idea of the individual's resistance: good physique, with extensive signs—a good resistance; a poor physique, with few signs—a poor one.

Before it is possible to treat a patient, it is necessary to ascertain his need for rest and capacity for exercise. This information cannot be obtained

alone from the examination of the chest, even in a febrile case, much less in one without fever. This statement is made because it is still believed that "moist" crepitations and such signs mean activity. I do not agree. The temperature, naturally, indicates rest, but that information is not furnished by the stethoscope. The only method of ascertaining the patient's efficiency for full exercise is to test him through graduated exercise or labour. In other words, having established the fact of infection of disease, it is essential to test the patients' powers of resistance to their own toxins, and so find out whether or not the disease is active by means of the patients' own exertions and not by auscultation.

A man may have active disease in that his lesion is connected with his blood fluids, but so long as he rests in a bath-chair, auto-inoculations do not occur, and the activity is not therefore demonstrable. But order this patient to get out of the chair and perform hard work, and so influence the bacterial products, his activity will then become apparent by the ensuing temperature. Whilst in the bath-chair he was apparently inactive, whereas the test exercise is irrefutable evidence of the activity of the disease; the physical signs will be found to be the same after the activity as when he was in the chair. Therefore the only way to establish whether or not tuberculosis is active, is by graded tests and not by auscultation.

XV

THAT THERE ARE THREE STAGES OF "PHTHISIS"

It should be self-evident that it is impossible to classify a general blood disease by its local manifestations. Yet practically all medical men still use this obviously obsolete classification. The local condition of the lungs matters little, so long as the resistance of the individual is high to the infection, and when no local conditions can be demonstrated, it is no consolation to the patient if his resistance to tuberculosis is either low or absent.

The exact rôle of the lung must be clearly understood in order to follow the absurdity of retaining such a nomenclature.

The lung is the local seat of the disease. The bacilli live and multiply there, and, in so doing, give off bacterial products. During their sojourn they actually destroy a certain amount of lung tissue. The destruction of the lung concerns the patient little, as there is such an enormous aerating surface of lung in reserve. I know of two men, who have been able to perform hard manual work, and who only had the use of one of their lungs. They each had a pneumothorax, so there can be no doubt as to the fact that both of them had one of their lungs out of action. One could not have a better example of the enormous reserve of lung tissue.

Once the lung tissues have been attacked

by tubercle, they are better out of the body with their accompanying mass of pus and bacilli. When this occurs a cavity results.

A tuberculous infection the size of a walnut at one apex is expelled from the body. This is a successful effort on the part of nature to get rid of diseased tissue and bacilli, and yet the patient passes from the first to the second stage, although he is really much better off than he was before the cavity had formed; yet by Turban's classification he is worse — could anything be more absurd? A surgeon always endeavours to remove the material formed by microbes. When the detritus is expelled by natural methods from the lungs, it should be manifest that it is to the patient's advantage.

I have known many patients with extensive signs and cavities (3rd stage) go through a course of graduated labour, and return home to work. On the other hand, there have been numbers who had had no signs, but bacilli in their sputum (stage 1), who have never been fit for any but the lightest exercise, and who have had to be discharged as more or less incurable.

The only classification that is of practical value —at the moment a diagnosis of tuberculosis is made—is, as to whether or not there is a reasonable hope of restoring a patient to his or her working capacity, *i.e.*, restoring them to full health. It is difficult to understand this method until it has actually been demonstrated what patients with abundant signs can do, and what failures one has with the cases with few signs but low resistance.

If there were to be a competition on the following lines, I would select my patients not with a few signs, but those with quite a fair amount of disease and with cavities, but who were in good physical condition; in other words, those who showed proof of good resisting powers. "Competitors to select 100 patients suffering from pulmonary tuberculosis, and to treat them in the year 1921, from January 1 to July 1. The prize to be awarded on January 1, 1927, to the competitor who produced the greatest number of ex-patients who had remained well, and at work, since their discharge."

The practice of judging cases by their physical signs has led to the use of that most erroneous expression, "an early case." According to this system, all people with a few signs ought to do well, and all those with many (advanced signs) should do badly. Whereas, the only method accurately to assess a case is, as has been indicated, by comparing the extent of the disease with the physical fitness of the individual, and then to test by exercises.

It is useful to repeat here part of the Report of the Medical Research Committee on V.D.H. "That a man's observed capacity to accomplish work of a given order is the only dependable test of his capacity would seem self-evident; yet it is the experience that Medical Officers rely more upon physical signs obtained while the subject is at rest." That is just what occurs in tuberculosis at the present time.

I shall never forget the case of a burly Scot—

a London police sergeant. He went to Brompton Sanatorium and did well until, unfortunately, he was laid low by a pneumo-thorax, followed by a hydro-pneumo-thorax. He recovered from this complication sufficiently to go through all the grades, eventually doing the hardest navvy work. He had a certain amount of dyspnœa, but very much less than one would have expected.

On his discharge he had all the classical signs of a hydro-pneumo-thorax. The heart well over to the right side; a good splash; complete dulness over lower half of chest; absence of breath sounds, and a well-marked bell note; the fluid was clear.

Before the time came for his discharge, naturally the question of his future employment was discussed between us, and I advised him to return to the Force. On his reaching London he went to his divisional surgeon, who pronounced him, as was to be expected from his physical signs, quite unfit for duty, and he was therefore referred to headquarters with the usual report.

The then chief surgeon was aware of my views on tuberculosis and he communicated with me, asking what had I to say about the report. I replied that the man was quite fit for duty, even though his heart was on his right side and his chest full of fluid.

He was accordingly returned to duty. Subsequently, he used to meet me at Brompton. His dyspnœa became troublesome, and, from time to time, I would aspirate to the extent of one to one-and-a-half pints of clear fluid. The disease was, of course, quiescent, and no risk of

an adverse auto-inoculation. I cannot conceive a clearer case of the misleadingness of physical signs considered by themselves. I wonder what stage this man should be called, and how would Turban classify him?

Having recalled to memory this patient, who was at Brompton Hospital Sanatorium for some eight months, and who was discharged in 1911, I thought it would be interesting to hear what had become of him since I had lost sight of him. Remember that physical signs had definitely said he was no longer fit for police duty in 1911. On making inquiries in 1919, I found he was still at work as a policeman, and had risen to the rank of superintendent through that of inspector, and was, in addition to his work, cultivating a half-acre allotment. He stated also that he was in good health, and that he had not been aspirated for eight years. I attribute his recovery and long life to the fact that the fluid in his chest was left alone whilst the disease was active.

A practically similar case occurred at West Wales Sanatorium several years later. Dr Watson left the fluid alone in spite of the great dyspnœa, and the man made a good recovery and returned to work.

XVI

THAT, CLINICALLY, IT IS POSSIBLE TO DISTINGUISH BETWEEN INFLUENZA AND PULMONARY TUBERCULOSIS

During the time I was Resident Medical Officer at Brompton, which period was antecedent to the theory of auto-inoculation, it was usual to classify a rise of temperature in an ordinary case of "chronic" pulmonary tuberculosis as influenza, even when there was no epidemic. Since acquiring some knowledge of the action of bacteria and their products in tuberculosis, I have taken many histories of patients admitted to sanatoria, and I have found that the precursors of the disease are three, and they are indicated in the following table, culled from the statistics concerning 5856 patients of the Phipp's Institute, on the admission of the patients:—

23·8	per cent.	had	already had	"pleurisy."
25·9	„	„	„	influenza.
47·4	„	„	„	hæmoptysis.
97·1				

The history is usually given thus: "I was quite well until I had influenza, and I've never been quite right since then."

What are the signs and symptoms of a recent case of auto-inoculation due to exercise? Headache, fever, pains in the limbs and eyes, photophobia, anorexia, and what is usually described

as "feeling rotten." Are these symptoms any different from those of the ordinary onset of influenza? They are not, simply because they denote, not a particular disease, but a toxæmia due to bacterial action. At the onset of an invasion by bacilli of tuberculosis or influenza, there is no clinical difference. Should the symptoms all disappear in a few days, the illness is put down as "influenza." This is an entirely wrong deduction, as has been shown elsewhere; a tuberculous auto-inoculation equally subsides in a few days.

Conversely, as demonstrated in the case of "once tuberculous, always tuberculous," because a patient has definite physical signs in the lungs, and in addition a rise of temperature, and the symptoms above mentioned, it is not evidence that the patient has active tuberculosis.

Surely there is no more emphatic testimony to the clinical difficulties of a differentiation between the two diseases when one group of clinicians describes a rise of temperature in a proven case of tuberculosis to be influenza, and another section terms an arrested case of tuberculosis with pyrexia an active case of tuberculosis. It is essential to make explicit the point at issue, namely, that after the acute stage is over it is possible, in some cases, to decide what *has been* the determining cause of the indisposition, but clinically, during the febrile stage, it is not practicable so to distinguish between the diseases.

Before the war, it was usual to classify a sudden onset of acute symptoms as influenza, and it is very easy to appreciate the reason. When a

person is taken ill, the patient's friends demand to know at once what is the ailment; hence it must be given a name, and "influenza" is a good enough term for the moment.

The war offered new opportunities of classifying diseases, and such an acute onset of illness would, in the Services, have been labelled "P.U.O." (pyrexia, unknown origin), until further attempts had been made to elucidate the causation of invalidism. This is a real advance in medical practice. It abandons the placing of a meaningless docket on a disease, and directs the energies in search of the source of the infection.

I can only affirm that I am unable clinically to differentiate between the dual conditions, and I am at present unacquainted with anyone who has the power accurately to form such a diagnosis solely by clinical methods. Placing on one side the definite but few epidemics of influenza, it undoubtedly has been the case that in normal times, cases of auto-inoculation due to the tubercle bacillus have been classified as influenza without further investigation. To put this in unmistakable English, it means that persons having been infected with the tubercle bacillus one day upset their balance, that is the ratio between the virulence of the invading bacillus, and the specific resisting power of the body to the particular organism, yet they nevertheless recover their equilibrium. If ever there have been or will be early cases, surely they stare us in the face, yet the opportunity has gone if they are designated "influenza," and the critical moment for treatment

is allowed to pass. True, the occasion for action will recur, but time and, possibly, the crucial opportunity, have been lost.

There is no research required here. It is a known fact, that what is at present called influenza is often tuberculosis. But the knowledge is not practically applied. It is information of inappreciable value lying idle. It is no new discovery, or its import would ring throughout the world.

If, therefore, the war alters the practice of considering pyrexia of uncertain origin as "influenza" without undeniable proof, and gives way to the better system of regarding "P.U.O." (pyrexia, unknown origin) as possible tuberculosis, until unmistakably ascertained to be otherwise, it will, from the medical point of view, have achieved much that is good, and we shall obtain "early cases" for treatment.

XVII

THAT YOU CAN DETECT ACTIVE TUBERCULOSIS BY MEANS OF THE "X"-RAYS.

The only visible way of detecting active tuberculosis by this method, would mean the observation as to whether bacterial products are or are not entering the blood-stream.

As has already been said, it is not possible to tell whether a carbuncle is adversely affecting the patient's blood or not by looking at it. Neither

can this information be obtained by the use of the "X"-rays. That "X"-rays will reveal pathological changes in the lungs is well known, and the extent of the lesion may also be ascertained. I have repeatedly pointed out that the extent of the disease is of little moment, once a diagnosis and prognosis have been made. As at present used, the "X"-rays are of little assistance. To make this quite clear: A carefully watched patient gives plain indications of progression or retrogression, which cannot in any way be ascertained from the visibility of the lungs.

XVIII

THAT HÆMOPTYSIS IN YOUNG PERSONS OFTEN PROCEEDS FROM VARICOSE VEINS IN THE THROAT

In taking the histories of patients suffering from proved tuberculosis, it is typical to obtain such a statement as the following: "I was quite well until some months ago, when I coughed up a little blood. I went to my doctor, who told me 'it came from some varicose veins at the back of my throat.'" I have heard this latter remark so frequently that I have come to the conclusion that medical men make such observations in what are believed to be the best interests of the patients, either to avoid frightening them, or in the hope that nothing further may happen. Cabot puts it

thus[79]: "The doctor allows his wish to be the father of his hope, and, humouring the patient's urgent desire to be told that he is not tuberculous, gives a clean bill of health, and surmises that the blood came from the throat." Whilst Norris and Landlis[80] writing on the same subject say: "In influencing physicians against a diagnosis of tuberculosis, the most important reason, in my opinion, is moral cowardice." To tell a patient that the blood comes from the throat, and leave it at that, is not in the best interests of the sufferer. It is far preferable to frighten him, and let him understand the truth. The blood spitting undoubtedly alarms the patient, and hence he is more amenable and willing as a rule to undergo treatment. He is incomparably more reasonable than, when in good health, he is told tubercle bacilli have been found in his sputum. He has neither seen nor felt the bacilli, and therefore does not appreciate their importance.

Hæmoptysis is frequently the first manifestation of the disease, and therefore reveals the onset of the malady. It should be realised that the individual feels in good health, has no symptoms, and therefore still has a high resistance; he thus has an excellent chance of recovery. But, unfortunately, how often is that golden opportunity for treatment lost, because no physical signs of disease are found in his chest, or because a few microscopically negative examinations of his sputum are obtained. What better evidence is there of tuberculosis than an hæmoptysis in a young person in perfect health, and yet to-day it

is not accepted unless bacilli are found. What else can it be in young persons?

The blood should always be inoculated into a guinea-pig. This method of confirmation does not receive the attention it deserves; a positive result being definite evidence of tuberculosis.

Cabot[30] sums up the situation admirably in the following words: "I do not deny that the causes of hæmoptysis are numerous, but I assert that the causes of genuinely obscure hæmoptysis, in a temperate climate, may be reduced to one—pulmonary tuberculosis." The control of tuberculosis would make rapid strides if every case of hæmoptysis in the young adult was considered to be due to tuberculosis until proved otherwise. This is the reverse of present-day medical teaching, namely, to consider it non-tuberculous until proved to be tuberculous — to wait and waste valuable time, with the patient figuratively sitting on the edge of a precipice.

F. Lord[31] states hæmoptysis occurs in 60 per cent. of all cases of tuberculosis.

Of 5856 cases examined at the Phipp's Institute,[32] hæmoptysis had already occurred in 47·6 per cent. of the patients.

The following table[33] shows the causes of primary hæmoptysis among Prussian soldiers, who were, until such an occurrence, apparently in good health :—

Tuberculosis . .	848	Influenza . . .	3
Trauma . .	11	Syphilis . . .	3
Pneumonia . .	7	Abscess and gangrene	2
Heart disease . .	5	Hydated cyst . .	1
Bronchiectasis . .	4	Irritating fumes .	1

The percentage in favour of this diagnosis is 96 per cent. When read in the light of Cabot's dictum, if we eliminate the "genuinely obscure," by taking out the eleven cases of trauma, seven of pneumonia, five of heart disease, and four of bronchiectasis, it is nearer 99 than 96 per cent.

As I have said, the chief error lies in thinking that because no physical signs are ascertainable, and on account of the patient feeling in good health, therefore the ailment cannot be tuberculous.

I have heard three reasons for not sending these cases for treatment: (1) that the patient may catch a worse form of the disease; (2) that a sanatorium is a depressing place; and (3) that the patient will get well at the sea-side.

The first excuse is untrue, and is dealt with under various headings. Numbers 2 and 3 will be considered fully under those titles, but I must state here that No. 3 is simply permitting the patient to continue, as I have said, figuratively to sit on the edge of a precipice; it is not treatment. The medical man does not exist who could take 100 cases of primary hæmoptysis and divide them into two classes—those which require treatment, and those which do not, and who would be proud of showing his powers of prognostication five years hence.

XIX

THAT HÆMOPTYSIS IS A SIGN OF ACTIVE TUBERCULOSIS

Pulmonary hæmoptysis due to tuberculosis is caused in two ways. Either by the direct action of the bacilli eroding the blood-vessels and weakening their walls, or to destruction of tissue around the vessels, so that they lose their external support. It is no uncommon sight at a *post-mortem* on a tuberculous cadaver, to observe a blood-vessel running through a cavity on which a small aneurism has formed. In both instances, the time arrives when the walls of the blood-vessels cannot withstand the pressure of the blood, and an hæmoptysis results. Clinically, there is no method of exact determination as to which of the two causes is the one actuating a particular hæmoptysis.

It is, however, a fair working policy to assume that an initial hæmoptysis in a young person, in whom few if any physical signs are discoverable, denotes the first class, and that an hæmoptysis in a patient who has been known to have the disease for some time, and who until the moment of the occurrence was feeling in perfect health and able to perform his usual functions, is due to the latter.

It is necessary to try to establish which is the cause, because the significance of the two causes varies widely. In the first class, living tubercle

bacilli are actively at work, the patient therefore has active disease, and his condition is calling for treatment after the conclusion of the attack.

In the second category, the hæmoptysis is obviously due to a mechanical defect. The surrounding support to the vessel has been removed, and an aneurism has formed. There is, therefore, a definite weak spot in the vessel. Once it has commenced to give way, it will continue gradually so to do until it bursts. There is no evidence here of activity of tubercle bacilli, although it is certainly the result of their labours. But such an event may occur months or years after their activities have ceased.

This form of hæmoptysis is fairly common amongst ex-sanatorium patients. As a rule, the bleeding usually ceases, if not in a few hours, in a few days, and the patients who recover are not only little the worse but are better for the event, because the aneurism has burst and the weak point on the blood-vessel obliterated. I have had many letters from ex-patients asking for advice after such an hæmoptysis. The correspondence is to the effect that their medical advisers counsel them to re-enter a sanatorium, which is contrary to the instruction given by me to them previous to discharge, to the effect, if they felt perfectly well both before and after an hæmoptysis, they were to return to their work without seeking further sanatorium treatment. Further, they would ask me to give them a note to their doctor to this effect. I have always complied with the request, and my advice has

been followed in a sufficiently large number of cases to convince me that it is absolutely sound.

Both these classes of hæmoptysis may be followed by a rise in the temperature of the patient; in the first class, it may be due to the active tuberculosis or the cause which occasions fever in the inactive cases.

As is well known in cases of severe hæmoptysis the patients inspire a quantity of blood. Indeed, in some instances, they are actually drowned in their own blood. This blood finds its way to the lung base and there sets up fever due to septic pneumonia, or to the absorption of the fluid which has left the circulation as blood.

In these cases, the resultant fever is accordingly not due to the tuberculosis but to other causes.

XX

THAT PLEURISY IN YOUNG PERSONS IS NOT DIAGNOSTIC OF TUBERCULOSIS

One has almost to repeat what has been written concerning hæmoptysis. An attack of pleurisy with effusion in young persons is almost certainly due to tuberculosis. The history given by patients who eventually have to seek treatment at tuberculosis institutions is frequently thus stated: "I was quite well until I had an attack of pleurisy, when I was laid up for some weeks, and I have never been quite well since." Exactly similar to

the histories given by the cases which begin as hæmoptysis. Like hæmoptysis in young persons, what is its probable cause? Practically nothing else but tuberculosis, unless it is called "idiopathic." I hope the war has killed that word, and when we do not know the cause of a pleurisy, we will admit it, as we have done by using "P.U.O." instead of the pre-war word "influenza."

Of 5895 tuberculous patients examined at the Phipp's Institute,[34] 23·8 per cent. gave an antecedent history of pleurisy. Norris and Landlis[35] state that the records of insurance companies show that the death-rate from tuberculosis of the lungs among persons who have had pleurisy within five years prior to insurance, is three times the average rate for individuals without such a history.

Every case of pleural effusion in young adults should be considered to be tuberculous until proved to be otherwise, and treated accordingly. If this were the practice, thousands of these patients would never have another breakdown, and the monotonous history quoted above would disappear, as it most certainly ought to do. Much is said concerning the need for research in the advancement of our knowledge concerning tuberculosis. What will be the advantage of information gained by research, if clinically it is neglected? The position to-day is, that pleurisy is treated as non-tuberculous unless proved to be tuberculous — absolutely the reverse of rightful clinical practice.

XXI

THAT IT IS POSSIBLE TO DIAGNOSE A PLEURAL EFFUSION BY PHYSICAL SIGNS ALONE

In most cases this is possible, but it is not an infallible method. The pleural cavity should always be explored when there is any suspicion of the presence of fluid. This can be done always without harm to the patient. This simple experiment settles at once, as to whether or not the physical signs are due to solid lung or pleural effusion; if the former, whether it is pus or serum, and if the latter, it supplies a sample of serum for inoculation into a guinea-pig, definitely to ascertain if the pleurisy is tuberculous. This test, however, is so seldom applied that one hardly ever hears of its use amongst the class of patients who come under the Insurance Act. The efficiency of the test is uncontroversial. It is the result of research, yet it is almost neglected. A good instance of information lying idle. In a definite case of tuberculosis should the fluid prove to be pus, it should be inoculated into a guinea-pig to ascertain whether or not it is tuberculous pus. An acute purulent pleural effusion in a case of tuberculosis seldom occurs. The fluid is nearly always clear at the onset, although it may afterwards be purulent. My endeavour here is to make it plain, that tuberculosis does not prevent a person having an ordinary empyema. It is stated further on in this book that a tuberculous empyema is

better left undrained. An ordinary empyema in a tuberculous person should, of course, be opened.

XXII

THAT SANATORIA ARE NO GOOD

I wonder how many medical men really know what they mean when they glibly make this statement. How many could define or properly describe a sanatorium for tuberculosis? Most of them would probably end up by calling it an "open-air place." If the successful treatment of pulmonary tuberculosis depended only on an "open-air place," there would be nothing further to say. Yet it is just this lack of knowledge which is at the bottom of this idea.

I think that the average practitioner must have in his mind the thought that if so-and-so just "leads an open-air life," all will go well. He has had little experience of sanatorium treatment on modern lines. He does not appreciate the difficulties of treatment, nor of the greater task of obtaining the co-operation of the patients in their own personal treatment. He has not come to realise that excessive auto-inoculations must be controlled, even if he considers their stimulation unsuitable for treatment. Why do these ideas hold the field? Partly from books and from tradition about the value of climate and open air, but chiefly from the so-called sanatoria.

It is obvious that the successful treatment of disease needs careful study, yet a great many members of the medical staff of the sanatoria of this country have undertaken sanatorium work without special knowledge or training, because they had, unfortunately, contracted tuberculosis in their practices, and after a period of treatment had turned to such work for the benefit of their own health.

Then there are the sanatoria where the patients spend the day in the "open air"; they rest or walk as they please. They are supplied with abundant food, are warned against over-exercise because of the risk of hæmoptysis, and in the end grow fat and lazy. All too often they become neurotic from having nothing to think about but their own symptoms and those of their fellow-sufferers. These are, indeed, "open-air places"—they certainly are not sanatoria. It must be admitted, however, that a certain number of patients do well even under such conditions.

The above treatment has been described as living a generally hygienic life, with over-feeding. There is, however, no effort to raise the specific resistance of the patient to tuberculosis, nor to test his resistance to his own toxins, nor to restore his muscular condition. Nor is any attempt made to teach the patients how to live afterwards, by giving them lectures or talks on the subject.

To an institution where these points were considered paramount—the leading of a hygienic life as the natural condition to keeping in health, and the absence of over-feeding, with definite action

in raising a man's resistance, coupled with education—to such an institution I would give the name of Sanatorium. The means of raising the specific resistance may be subcutaneous tuberculin treatment, or auto-inoculation, but since we know resistance can be raised or lowered, the means of controlling it must be there. How many such institutions, therefore, can be classed as sanatoria? Not many. The truth being that many "homes for tuberculosis" exist, but few sanatoria. It is because of such want of treatment that many unkind things are said of real sanatoria.

I am quite willing to admit that the treatment in an "open-air institution" or home for tuberculosis is often bad for the patients, for the reason given, namely, in making them chronic invalids.

I am equally prepared to admit that no sanatorium treatment is a specific; also that there are numerous failures, even under the very best possible conditions. It is, however, the best we can do at present.

Why do we expect always to have complete success when treating tuberculosis? Do we anticipate it from other forms of treatment? Do we, for instance, expect it from the treatment of cancer? Yet we keep on doing all we can for cancer, though we make little if any progress. Nobody complains of the money spent on investigating cancer, but many find fault with spending money on tuberculosis.

We know a great deal about tuberculosis, and much of this is due to sanatoria. Walther taught us that vigorous exercise was not dangerous.

Others, that pure air was not harmful, but beneficial; that over-eating was not necessary; that the blood fluids are influenced by rest and exercise; that exercise, of itself, does not necessarily give rise to hæmoptysis, and that tuberculous patients need not remain chronic invalids, as also many other valuable lessons. In fact, we have made much progress, except in the discovery of a rapid and reliable specific. If we give up sanatoria we surrender this valuable means of gleaning further information.

Those who make the statement that sanatoria are no good, altogether overlook the inestimable advantage enjoyed by sanatorium (not "homes for tuberculosis") patients who, having learned the benefits of the hygienic life, and who return home, see to it that their families adopt a rational method of living. I know that many medical men say they do not do this: my experience has been otherwise. It all depends on the hygienic education, and the INFLUENCE of the institution on the individual.

All who attended the Brompton Sanatorium annual garden parties, must have been struck by the healthful looks of the ex-patients and their relatives, who came down with them. When statisticians talk of assessing the results of sanatorium treatment, by arriving at statistics as to how many per cent. of discharged patients are alive five years after leaving the institution, they overlook that enormously valuable asset to the community, which is unassessable by such calculations, namely, the education of the community by ex-patients.

Let me now deal with those who state that statistics prove sanatoria to be unsuccessful as a weapon against tuberculosis. These statistics as often as not refer to "homes for tuberculosis," but that point need not enter into the argument. First, the statisticians do not appreciate the difficulties of keeping in touch with ex-sanatorium patients. In my experience, the working-class patient who does well after discharge too often completely loses touch with the sanatorium, so that there may be no stigma of the disease attaching to him, to his detriment. It may cause him to lose his job, or to drop in wage value. Consequently, a great number of the successes do not appear on the records.

On the other hand, those patients who relapse are only too glad to write, with a view often to further help, while those who die are usually reported by their relatives, or information is received from their late fellow-patients. My experience, therefore, of statistics is that you obtain more of the bad than of the good results concerning untraceable persons.

But even this fact need not enter into my argument. How many patients per cent. ought to be alive five years after a course of sanatorium treatment, and remain useful citizens? I believe that, given the opportunity of getting the patients for treatment before their resistance had broken down—that is, on their first attack of influenza—we should procure about 80 per cent. of permanent results. Doubtless, I shall be accused of being optimistic. Some think 60 per cent. is much

nearer the mark. To be absolutely on the safe side, let us assume therefore that, after five years, it is found that only 30 per cent. of the patients discharged from sanatoria are alive and well.

Now, here is a concrete fact to face. Are we willing to abandon sanatorium treatment for all suitable cases because only some 30 per cent. remain well? It is the question of Lot's wife over again. For how many righteous men will you save the city? Is it worth while going to this expense for 30 per cent. of lives saved? I would ask, is it worth while having lifeboats all round the coast for the few lives per cent. saved of those who sail the seas? Is it worth while having fire and life-saving appliances, considering how few times per cent. they are needed for the lives in danger? It is simply a question of degree as to what is worth while.

Finally, and to bring the matter home, supposing that it is agreed that it is not worth while. Then, in most homes must be witnessed the spectacle of husband or wife, sister or child, becoming infected with tuberculosis and allowed to die, because it is not worth the cost of treating one hundred persons to save thirty. That is what the tirade against sanatoria amounts to. To those who continue to say that sanatoria are useless, I would say, Is there any better treatment? I know of none. If we renounce all residential treatment of patients we give up many opportunities for research, and certainly we can make little progress without in-patients. Surely, the only rational view to take is, that so long as this

is the best that can be done for the patients, it is essential to continue to do it, until such time as we discover a better method. Nor must we forget that there are many channels for making reformed use of our present knowledge of treatment and diagnosis, and there certainly are plenty of opportunities for many critics to put their own houses in order. Whatever persons with little or no knowledge of sanatorium treatment may say, I am entirely convinced that real sanatorium treatment is an essential part of any scheme for the eradication of tuberculosis. Indeed, it stands in the relation to such a proposition as does a shackle to an anchor and cable; both are absolutely indispensable, but utterly useless for mooring a ship without it. As the shackle alone will not anchor the ship, so sanatoria by themselves, without all other means of assistance, will not eradicate tuberculosis.

XXIII

THAT TREATMENT BY MEANS OF AUTO-INOCULATION IS UNPRACTICAL; THAT IT IS DANGEROUS AND ONLY AN UNPROVED THEORY

One has heard such and similar statements made, over and over again, by those who are unacquainted with auto-inoculation as conducted in a modern sanatorium. Medical men have come to me frequently, in a more or less doubting mood,

to see graduated labour treatment, and have departed feeling that, having grasped the practice as well as the theory of auto-inoculation, they see pulmonary tuberculosis is a new light—a different disease from their previous conception—a blood disease and not a lung disease. Until all medical students understand tuberculosis from this point of view, real progress with the treatment and eradication of the disease must, more or less, mark time. This is a strong statement to make, but it is nevertheless true of such treatment.

For fuller details I refer the reader to *Auto-inoculation in Pulmonary Tuberculosis*,[86] but the three outstanding facts which arrested the attention of the medical investigators mentioned above are :—

(1) A rise in the temperature after increased exercise, and the fairly easy control of the temperature by means of complete immobilisation: that is a large auto-inoculation, which is properly controlled, and the improvement which is obvious a few days after such an event.

(2) The influence of exercise and rest on the sputum, in varying its quantity and character. One grade may greatly increase the quantity, and it may remain so for several grades, until it finally disappears, perhaps after a rise of temperature, or possibly on the final, hardest work.

(3) The subnormal deviations of the temperature.

It is well to emphasise the observation of sputum from the quantitative standpoint. More attention at present is paid to gain of weight, but

more information would be gained if 5 per cent. of attention went to weight and 95 per cent. to sputum.

A surgeon watches the discharge from a wound; when the discharge is abundant, he knows that healing will not take place soon; but, as the pus decreases, he perceives the time for the closing of the wound has arrived. Sputum is the pulmonary pus, or wound discharge. If it increases, there is more activity of the microbes; and as it diminishes, on the final, hardest work, it gives evidence that the disease is abating. This is surer information than can be obtained by listening with a stethoscope. What would be thought of a surgeon who, after an operation for an empyema, took a stethoscope to examine the affected side, and who paid little attention to the amount of discharge on the dressings.

The practice of auto-inoculation has suffered from the methods employed in its use by some practitioners. There are those who regard graduated labour as "a very good idea, as it keeps the patients' minds employed, and so off their illness," and they use it accordingly. This means that, as a general rule, the patients are turned out to "do a little gardening." They are left to themselves—hence they treat themselves unwittingly by auto-inoculation. Some of the lazy ones do nothing at all, and suffer no harm. The keen patients do too much, and may injure themselves. To turn a patient out to do gardening is not necessarily to "grade" him. Whereas, if he is properly graded, and made to keep on that grade,

although he may perform work which will cause a rise of temperature, he will do himself no harm.

Other sanatoria go further and grade their patients, but they do not go far enough. They leave the selection of the actual task in that grade to a layman, whether he be gardener, labour-master or what-not. He is keen, therefore, on labour, and not on the treatment of the patient. If, then, a patient is left to such a layman, the latter will employ him as seems most useful to the institution. So that a man who is a bricklayer will probably be kept laying bricks through all the grades of exercise, to the benefit of the institution, but the detriment of the individual.

Then again, the lazy patient will idle and the industrious one will work. The patients, naturally, do not look upon the graduated work in the same way as when the whole of the directions, in all details, are given them by a medical man, and he sees that they carry out what they are told to do, and compels them to conform to it. It is difficult sometimes to make them do it at first, but they soon realise how much benefit they are receiving, and those who are worth their salt become willing to work as they are told.

The number of sanatoria working truly on the lines of auto-inoculation is small, that is, where a medical man attends all details of work, and they must not be judged solely by those whose patients "do a little gardening."

The statement that auto-inoculation is dangerous is easily answered. I have treated thousands of patients on the lines indicated, and have never

had one refuse because "it was dangerous." Are they not the best persons to judge? Is it not their health or life which is at stake? Is it to be supposed that if it were hazardous, it would have been possible to induce manual workers to undergo treatment by these methods?

It is difficult enough to convince manual workers, such, for example, as colliers, that, by pick and shovel, they will find their own salvation from their affliction. It would have been absolutely impossible to induce them to work unless they had been confident that it was for their benefit, let alone there being any suspicion of jeopardy in it. Yet I have never had a collier patient or other refuse to work because of the supposed danger from working. This, surely, is the answer. I have never known any medical worker in a sanatorium to have that fear after a few months' experience. Remember that I suffered in the early days of Frimley from the fear that hard work might cause increased activity of the disease, and it is only natural that men trained on the traditions of the present medical text-books, should be afraid of the effects of this treatment, until they accept such statements as I have made, or verify them for themselves.

One of the first institutions to adopt auto-inoculation was the Loomis Sanatorium, New York, U.S.A., where it was introduced about the year 1908. The following is an extract from the report of Maxon King[87]:—

"If any one measure, more than another, is

responsible for this betterment in the results attained from treatment, it is probably the more vigorous and better organised system of graduated exercise, which has been employed in the treatment of all suitable cases. . . . During the past year, in response to a request from the more intelligent of the patients themselves, who have observed the rather spectacular benefits following 'auto-inoculation' by manual labour among the patients in the annexe, such patients as presented suitable physical condition, were organised into squads, and given exercise in the form of manual labour—precisely on the same plan as that employed in the annexe. It was, of course, carefully graduated, and the whole kept under a strict medical supervision."

There are several points to be noted. They are obtaining good results. Why? They do not send patients to "do a bit of gardening." They have a "VIGOROUS AND BETTER ORGANISED SYSTEM," and finally, "IT WAS . . . CAREFULLY GRADUATED, AND THE WHOLE KEPT UNDER A STRICT MEDICAL SUPERVISION."

The following is taken from the Annual Report of the same sanatorium,[38] published two years later. Note here again that they obtain good results, and they having tried it, believe in the theory; and note also their words: "GRADUATED TO THE CONDITION AND PROGRESS OF THE INDIVIDUAL, AND ALWAYS UNDER STRICT MEDICAL SUPERVISION."

"The employment of graduated exercise for the purpose of producing controlled auto-inoculation, is variously accomplished in all divisions of the sanatorium, by means of walking, and particu-

larly by systematic manual labour graduated to the condition and progress of the individual, and always under strict medical supervision. The results of auto-inoculation, as applied to the main division of the sanatorium, have been exceptionally gratifying during the last year, but in considering these results it must not be forgotten that the patients who qualify for such treatment are carefully selected, and naturally present a more favourable prognosis as a class."

If any further plea be required for auto-inoculation, let me ask the following questions:—

Is not graduated labour better than lying all day in a chair? Is it not better to keep patients employed, instead of making invalids of them? Is it not better to turn them out with their muscular system in full working order, instead of in a fat, flabby condition? And, finally, is it not better from a psychological point of view, to send them home *knowing* that they really can perform hard work? By graduated labour all these things can be accomplished, and, at the same time, the patient's resistance to his own toxins raised to such an extent as to start the healing of the local lesion in his lungs.

To show that there is evidence that auto-inoculation is not understood by many sanatorium workers, I quote the following remarks of Kinghorn,[89] an American sanatorium physician, written in 1919, who criticises my methods: "Work therapy should ONLY BE CONSIDERED for the positively arrested form of fibrous phthisis." This remark demonstrates clearly that he has not in any way grasped what I have advocated, namely,

the use of the patients' own bacterial products to attempt the arrest of this disease. This, obviously, cannot be done if the disease *is* arrested. It could not be possible to have a better example of the want of appreciation of the word "auto-inoculation."

XXIV

THAT TREATMENT BY TUBERCULIN IS BETTER THAN TREATMENT BY AUTO-INOCULATION

Those who disparage the use of auto-inoculation in tuberculosis, say: "You do not know the proper dose; you have no control over it, and you are using living bacilli, which is a dangerous procedure." Before considering the criticisms in detail, let me capitulate the points in favour of auto-inoculation when compared with inoculation by tuberculin.

The patient's own tuberculin is being utilised instead of a stock tuberculin.

Hunter was, I believe, the first man to advocate that real medical practice was to endeavour to assist Nature in her efforts to heal. All that has been discovered since, in the medical world, has proved his ideas to be sound. All those persons who recover from tuberculosis, as described by Nageli,[16] without being aware of their ailment, owe their recovery to auto-inoculation.

Auto-inoculation, consequently, is Nature's own method of treatment. When we employ it,

we simply use our knowledge to co-operate with her. This fundamental fact is, unfortunately, not yet realised.

The patient's muscular condition is being restored to normal conditions;

The patient is also, at the same time, being tested by work as to his suitability for the continuance of treatment, and finally as to whether or not he is arrested;

Metabolism is increased.

To those who state that the dose is an unknown quantity and that, therefore, there is no control over it, I reply that in practice, though not in theory, the statement is entirely wrong. If it were not so, patients would not in a body submit to such treatment; they would rapidly discover our ignorance.

It is necessary to emphasise that there will be the difficulties suggested if the exercise is not graduated, and if any rise of temperature due to a dose of bacterial products is not controlled by complete immobilisation.

The next point to study is the subject of living bacilli. Liebermeister[27] has demonstrated that bacilli are found in the blood. In the two cases quoted by me, in which febrile patients infected the guinea-pigs, the patients were dealing with living bacilli in their bloods. The blood, in cases of hæmoptysis, is also known to be capable of infecting guinea-pigs.[43] So there can be no doubt that living bacilli enter the blood. Consider the case of a person with latent tuberculosis who undergoes some unusual exercise—for example,

a long bicycle ride—and who develops pleurisy as a consequence. He must surely be dealing with living bacilli in his blood in order to carry the infection to the pleura. How many of such pleurisies are dangerous? They, practically, all clear up, in spite of the fact that there is no control of the dose. What grounds, therefore, are there for such a statement? There is no proof at all that the presence of tubercle bacilli in the blood is of itself dangerous, and there certainly is evidence that they have been present in patients who have done well.

Trudeau[40] conducted some very interesting experiments on guinea-pigs and rabbits. He came to the conclusion that animals inoculated with living but attenuated tubercle bacilli, showed a much greater immunity than those inoculated with bacilli killed by heat. This is certainly evidence in favour of using the patient's own bacilli, rather than of those introduced from without.

The experiments on animals by Webb and Williams[41] showed that monkeys could be gradually inoculated with living bacilli until they succeeded in giving to two monkeys, with safety, a dose of virulent human tubercle bacilli sufficient to kill at least 12,000 full-grown guinea-pigs. They also report having given virulent tubercle bacilli to some 40 "volunteers" for treatment. They say: "We have not inoculated beyond 5000 bacteria. In general, the patients have been in Turban's class 3; the results will be given later." They are of opinion that some good has been

done, but certainly no harm, and they expect better results with larger doses. They also report later,[42] "That two children have been successfully vaccinated with upwards of 600 virulent tubercle bacilli, without infection being produced to date." Four years afterwards these children gave negative von Pirquet tests.

It is well known that the virulence of bacilli differs. Webb and Gilbert[77] found that they could not immunise monkeys to all strains of bacilli, as some were too virulent. In giving a stock vaccine for tuberculosis, one does not know that it is the right strain. That is, that it is suitable for the virulence of the bacilli of the patient. In the case of auto-inoculation, there is no question about it. It is true that an autogenous vaccine can be prepared, but there remains as animal evidence in favour of auto-inoculation, that living bacilli are better than those killed by heat, and the raising of the monkeys' resistance to tuberculosis, to stand such enormous doses of living bacilli. I am not aware that similar results have been obtained by dead bacilli. Brownlee[86] in his report on the epidemiology of tuberculosis in Great Britain, discusses the varieties of tuberculosis, and writes as follows: "From analogy with other diseases, it would seem that if any vaccine or serum treatment is adopted, it will be necessary to see that the vaccine or serum is made with the proper organism. It is useless to protect against paratyphoid fever with an antityphoid vaccine. It can thus well be imagined that treatment with tuberculin will have little or no effect if . . . there

are differences between the various strains of tubercle bacilli, comparable to that found between the organisms of typhoid and paratyphoid."

As auto-inoculation is the natural method of healing, if there is any alteration of strain there is an immediate alteration of the bacteria and their products passing into the blood-stream.

Nothing needs to be said in favour of the advantage of restoring the patient's muscular condition. Apart from food, little is known concerning the effects of the increase of metabolism in tuberculous persons, which is, of course, very great under auto-inoculation treatment. Personally, I believe it will be found to be an important element in treatment. We are all aware of the effect of exercise, followed by proper rest, on appetite, and the benefits of even a short holiday under these conditions, of increased metabolism.

In addition to the advantages enumerated, the patient is being trained not to be AFRAID of work, and to lead the life that will help to keep him well after his discharge.

Finally, it is only by test exercises that we can determine arrested disease; there is no other method, and it is unnecessary to repeat here what has already been written on the subject.

XXV

THAT PATIENTS WITH A CAVITY ARE NOT SUITABLE FOR TREATMENT BY GRADUATED LABOUR

Several years ago I was consulted by the father of a girl who had fairly extensive tuberculosis and cavities, but her general condition was good. He informed me that he wished she could undergo treatment by graduated labour, but he had been informed that it would kill her. The person so informing him, had, himself, never treated anyone by graduated labour. I did not agree with the verdict. It was eventually arranged that the patient should go to a sanatorium run on strictly auto-inoculation lines. I made no communication to the medical superintendent as to her supposed unfitness. He shared my views, however, as she went through all the grades, eventually performing the hardest manual labour. It did not kill her, but greatly improved her health, and she was alive when last I heard of her, some five years later. It is necessary to repeat that physical signs in the lungs are not the only criterion by which a patient is to be judged. Of far greater importance is the patient's resistance to the disease. This can only be determined by exercise tests. Cavities, themselves, are no contra indication to such tests.

XXVI

THAT A SANATORIUM IS A DEPRESSING PLACE

There is absolutely no reason why this should be so. Probably, the medical men holding this view obtained their information from patients who had been to a "home for tuberculosis." In a sanatorium, run on auto-inoculation principles, the usual complaint from the patients is, that they have not sufficient time to themselves. Theodore Williams, in 1908, stated that "No one who visited the Frimley Sanatorium could help being struck by the cheerfulness and liveliness of the inmates."

Graduated labour has a moral value that is difficult to over-estimate. Many patients, when they start treatment by exercise, are noticeable for their sullen and apathetic attitude; as soon as the bacterial products become influenced, and their physical condition improves, all traces of gloom and depression vanish, and they become transformed into cheerful and lively persons. Out-door work is, of course, an admirable antidote to physical and mental disorders: it is a healthy occupation, from all points of view; it strengthens the body, and diverts the mind. But the value of graduated labour to patients is much more than this. When it is remembered that the average patient, going through the treatment, spends a week on each grade of walking—a week on the first three labour grades, a fortnight on the

next, and three weeks on each of the two succeeding grades, it might well be supposed the process is monotonous and dull. The reverse, however, is the truth. To the patient who desires to regain his health the grades are definite steps in his recovery, which he is slowly but surely mounting. As the days pass, he observes the diminution of his sputum, he becomes aware of increased physical fitness, and realises, without medical prompting, that he is making steady progress. Even should he receive a large dose of bacterial products, and have to retire to bed, he knows he will start again on the step where he left off, and not on the bottom rung of the ladder. There can be no depression or monotony to a patient who is aware that each fresh grade he reaches means a higher level of improvement, and who sees around him, in his fellow-patients, practical object-lessons in the successful results of the treatment.

Perhaps the best evidence of the absence of gloom in a sanatorium, may be obtained by reading the following topical allusions to sanatorium regimen. These verses were written by a patient, and recited at a concert entirely organised by patients, "The Doctor" being, as usual, the chairman, the author, of course, knowing well that he would be there in that capacity. It will be observed that there is a lively appreciation of the difference between sanatorium treatment and "a little gardening," and the fate awaiting "slackers."

In the weekly lectures, the effects of auto-inoculation were summed up in the word "poisons."

A SANATORIUM RHYME

When you've walked around the field a dozen times or more,
And the rather bumpy pathways made your feet feel very sore,
As you watch the winding highway a-stretching out below,
Don't you feel that you would very greatly like to know—
Just how many little T.B.'s a-wishing you would rest,
You have given so much poison that it's sent 'em on down West?

Or, when baskets you have carried for many a weary day,
Till your muscles ache like fury, and you feel inclined to say
"Of sanatorium life enough I've had. I'll hie me hence,"
Whilst the T.B.'s are rejoicing at the end of your defence.
It's comforting—now isn't it?—to say "Oh! no, I won't!"
If those T.B.'s think they'll have their way, I'll just see that they don't.

Or, when you've been turf-lifting, and the sun is very hot,
And suddenly you do espy a lovely shady spot,
If you yield to strong temptation, and lie down in the shade,
And think, well this, at any rate's a very "cushy" grade;
Don't you wish that Doctor took a nap 'twixt three o'clock and four,
When you stand, in fear and trembling, outside his office door?

Or, when you're feeling "rotten," and your food you cannot eat
And Doctor sends you down enormous quantities of meat.
Because you cannot eat it—to the office you must go,
And he, hard-hearted person, says things that make you sore,
Some comfort still remaineth, as you clutch your burning head,
For those little T.B.'s murmur, "Cheer up! We'll soon be dead!"

IRENE JONES.

XXVII

THAT THE SUPERINTENDENT OF A SANATORIUM HAS A "CUSHY" JOB

Those of my friends, who, previous to the war, had been engaged in tuberculosis work, and who joined the Navy and Army, tell me that in the Services there is a prevalent belief that many of the Medical Officers hope to eke out their pensions by obtaining a post as medical superintendent of a sanatorium.

It is necessary to repeat that for many years it has been the custom for medical men, who, owing to tuberculosis, had broken down in their work, on their recovery to start sanatorium work, irrespective of what were their qualifications before that illness. These two facts indicate the prevailing idea amongst the profession, that no special attributes are necessary for such work. The only treatment that has been absorbed by the minds of the majority of medical men is "fresh air and plenty of food, and anybody can look after patients undergoing such treatment."

Then there is the "no-treatment-at-all school," who send their patients to the sea-side or for a sea voyage, as will be referred to later. Under these circumstances, there is little wonder that many medical men regard sanatoria as homes for tuberculosis. Worst of all, what are merely "homes for tuberculosis," are called sanatoria. The word sanatorium has, unfortunately, become a synonym for any institution for tuberculosis.

I use the word sanatorium solely as indicating a place where patients are received *for treatment*, and that only those patients are there received who have a reasonable prospect of being restored to their full working capacity. I do not consider that an afebrile case is receiving treatment when he is lying on his back in the country, drinking milk. That was the prevalent idea when I was asked to inaugurate the Brompton Hospital Sanatorium at Frimley, and I hesitated considerably before accepting the appointment, because the idea of spending my life at the head of a "home for tuberculosis," did not appeal to me at all. It would, indeed, have been a comfortable job, as things then were.

I have since frequently wondered as to whether or not it was a pity I did not "let sleeping dogs lie," instead of creating a rod for my own back, as I think the following narrative will show.

To treat patients by auto-inoculation successfully, a medical superintendent requires the following attributes :—

An infinite capacity for taking pains and paying attention to detail.

A cheerful, optimistic temperament (he must have perfect health or he certainly will not possess these qualities).

The "patience of Job," and a capacity for working twelve hours a day, seven days a week. If the sanatorium is large enough for more than one doctor, he will have easier hours ; but his back will still carry the sanatorium.

A knowledge of all kinds of work, including

gardening. As a town-bred man, I found the latter as difficult as physiology.

A good memory, so that he may recollect all details concerning each of his patients,* and never betray a sign of ignorance or he may lose their confidence.

A strong personality—one which will compel his patients to do what is right, even against their will.

Neither an attack of tuberculosis nor service in the Navy or Army necessarily endows a man with all these qualifications.

Here I may interpose that, from personal experience, seventy patients is the GREATEST number that a thoroughly-trained and efficient medical man can undertake to successfully handle on auto-inoculation principles. Personally, I have, during the war, controlled one hundred and ten, and I make the above statement, not from theory, but from actual experience.

Details of a week's work in a sanatorium supervised by one man, are as follows :—

Beginning work at 9 A.M., there are the bed-patients to visit, as well as those suffering from minor ailments, and this duty, with the dispensing, usually occupies about an hour. One day in the week this hour is given up to prescribing the necessary grades of exercise for the patients.

From 10 to 12 o'clock are the patients' working hours, and it is essential to spend all this time with them to make certain that they each

* When patients have to be numbered or badged, this knowledge is wanting.

perform their respective tasks, and to encourage them, especially during adverse conditions of weather. When it rains, it is obligatory for the superintendent to remain with them, or they will not "stick it." If he does not shepherd them always, one unruly member will say, "What rot it is," and will lead the other patients to give up working, and therefore treatment. But if the superintendent will, without appearing bored, go through the same routine day after day, he will induce the patients to follow his advice implicitly. It is necessary to chide some and laugh at others. He must be very human and understanding.

From 12 to 12.45 those patients are interviewed who have committed offences. An eye is at the same time kept on those patients who are soon to be discharged, and who work through this hour. The other eye must be kept on the patients who are expected to be resting at this period, to see that they do not write letters, sort out their lockers, or do anything else but rest. Nurses abhor this essential police work, and few will undertake it. This hour includes his interval for luncheon.

From 1 to 2 o'clock the principal meal of the day is served. The superintendent's presence stimulates the patients to eat their meat, and puts the seal of science and sign of medicine over the meal: he sees to it that they clear their plates. And this can readily be accomplished so long as the patients appreciate that their "doctor" is fully conversant with their treatment and all that is required of them. As long as he has a hold on

them, so long will they follow where he leads. From 2 o'clock to 2.30 he can have a rest and a smoke, again with one eye open to see that nobody gets up to mischief. Here again, few nurses are good as policewomen. The patients are mostly young; many feel better than they have ever been in their lives, and they get skittish and full of pranks. All such have to be duly suppressed, though with understanding and good feeling. If untactful measures are used, patients will leave the institution. One is liable to lose two classes—those who lose their tempers because of the tricks played on them, or the trickster, from bad handling by the superintendent.

From 2.30 P.M. to 4.30 the same routine is followed as in the morning. Tea is served from 4.30 to 5 o'clock, and the man has a rest. From 5 to 6.30 is occupied either by examining patients or lecturing to them. At 6.30 supper is served, and at 7.30 the medical superintendent has his dinner. At 8.30 come the night rounds—these are essential, and a preventive measure, as otherwise patients are once more apt to get into mischief. To give an example of the worst form of indiscretion, I have known patients get up, dress, and go to a public-house. Let me reiterate that through all this the superintendent must still be optimistic and good-tempered.

Then on Sunday, a general observation must be kept on the patients. A sanatorium should not be a prison, or the patients will not remain. It is neither possible nor advisable to attempt to control them by locks and walls, they must have

the feeling of freedom; also, it is necessary for the medical superintendent to keep the strictest discipline on Sundays as well as on week-days. It is like the training of young horses—having trained one batch, a fresh start has to be made the next week with the new arrivals. A patient undergoing treatment by auto-inoculation, should not on Sundays undertake more exercise than is allowed on week-days. This necessitates much supervision of the patients. They do not do wrong so much wilfully, as from carelessness or from want of understanding. All these points have to be understood and dealt with by the man in the so-called "cushy" job.

And in the midst of all this tedious routine, the superintendent often has his heart wrung by one of his best patients returning to the sanatorium in such a state that all can smell where he has been. Of course these two have to part. The whole week, indeed, is one of continual watchfulness, and keeping everybody up to their task; if the superintendent is slack, so is the auto-inoculation.

Unfortunately, the number of men who are able to work in this way is few, and still fewer perhaps are those who try to UNDERSTAND their patients.

There are sanatoria run under the Insurance Act, which, so far as sanatorium buildings go, are superior to anything I have seen, for those who can afford to pay for treatment, but which have medical superintendents who do not spend all the working hours with their patients; hence their patients never have efficient treatment by auto-inoculation.

We come back here to the bed-rock of all successful treatment, as has been shown by Walther of Nordrach, that it is not the buildings of a sanatorium that matter, *but the man in charge of them.* With all the money being spent under the Insurance Act, I am afraid but a very small proportion is being spent on SANATORIUM treatment. Most of the money goes to the soft jobs—the "Homes for Tuberculosis." Hence this idea.

XXVIII

THAT SANATORIA SHOULD BE BUILT OF WOOD, SO THAT THEY CAN PERIODICALLY BE BURNED DOWN

I cannot conceive of any person who, knowing anything of a properly conducted institution for tuberculosis, could make such a statement. That it should be repeated, is clear proof of what I have previously stated concerning the existence of the haziest ideas on sanatoria and their methods. It is essential to conduct a sanatorium in such a manner that it will neither reinfect the patients nor infect the staff. Were it otherwise, they could not exist. On the one hand, unless the patients received benefit instead of harm, they would not remain in such an institution, and on the other, the staff necessary to deal with the patients would be unobtainable, for obvious reasons.

During my fifteen years' connection with the Brompton Hospital and its sanatorium, I recollect

only one member of the female staff exhibiting evidence of tuberculosis, and she had a history of previous tuberculous illness in her childhood. During the time I was at Frimley, between 15 and 20 per cent. of the women patients were nurses from the London general hospitals—but not from the London "consumption" hospitals. Very good evidence of the safety of a tuberculosis institution.

In the Departmental Committee's Report on Tuberculosis,[45] signed by every member of that Committee, occurs this statement:—

"The Committee desire to place on record their opinion that a properly conducted institution is not a source of danger to the neighbourhood."

I think I have said enough to demonstrate the absurdity of the statement as to the periodical burning down of sanatoria. But the authors of the dictum also exhibit a complete want of vision and great narrow-mindedness. The patient does not infect the sanatorium, because he is trained not to do so. Patients in their homes are usually untrained, and therefore infect their houses. If there is to be any burning, surely it is the domiciles of the tuberculous that should be so sterilised. I have not, however, yet heard anyone advocate this measure, although it is the more logical of the two suggestions. But both are equally silly.

XXIX

THAT THE WARDS OF A SANATORIUM MUST FACE THE SOUTH

If all the patients were bedridden, there would be good reason for this dictum. As, however, the majority of the patients should be up and taking exercise during the day, if it is a sanatorium and not a home for tuberculosis, there is hardly any reason for the expense such a procedure incurs. If sanatoria are built with wards on each side of a central corridor, it is cheaper than having them on one side only, and just as efficacious. I have worked in sanatoria built on both plans, and I have not observed any difference in the progress of the patients, nor have I found that there was any preference, on their part, for the south wards, even during a very severe winter, when the desire for the warmth of the sun should certainly have become manifest, if there was any advantage to be obtained.

XXX

THAT SANATORIA ARE HARMFUL

Louis Cobbett[46] makes the following comment on sanatorium treatment.

"Admitting that some cases have been cured outright . . . is not this advantage more than

counterbalanced by the number of those who would otherwise have quietly died, but who, through the help of sanatoria, have continued to live for years, to be a danger to their neighbours —sanatoria, then, take away with one hand what they give with the other."

He may be right or he may be wrong, but he overlooks the reason why such cases are a danger to their neighbours; it is not their fault, but, as I have already explained, the fault of their neighbours, and it is certainly hard on sanatoria to bear the blame for the indiscretions of others. I have had experience of tuberculosis for twenty years—not merely visited it, but LIVED in the midst of it, and, naturally, have seen all its forms. I have one hard, firmly rooted conviction, namely, that one is safer in a properly conducted tuberculosis institution, so far as tuberculosis is concerned, than one is anywhere else. Patients do not infect their attendants, because the latter encourage the use of sputum collectors. Tuberculous persons contaminate their neighbours, because the latter discourage the use of sputum flasks.

Cobbett's statement means, in plain language, that one of the surest ways to stamp out tuberculosis would be to hasten the deaths of all tuberculous persons. There can be no doubt that to kill every person having tuberculosis would be quicker still. What we have to face are not improbabilities, but the present position of affairs, and the feelings of living men and women, both those suffering from the disease and those who are not. Is it a tradition of our race to hasten the

death of other people? Is it the inclination of our kindred to suffer death willingly for the sake of the community? Having considered these questions, is it probable that public opinion would agree to abolish sanatorium treatment, so that men and women might die the sooner?

The only way to deal with tuberculosis is by practical methods; so long as there are tuberculous patients, so long will their friends demand treatment. As a nation, it is not credible that we shall be willing to surrender either our lives or our freedom. As such drastic measures are impossible, is nothing to be attempted?

To abandon sanatorium treatment means that we cease our efforts, and merely become onlookers. I am afraid this idea is resident in a good many minds. The true position of affairs is that there are few sanatoria but many "homes for tuberculosis," and that, in consequence, true sanatoria have never had a fair trial, and that the patients with tuberculosis have not been diagnosed in the earliest possible days after infection, because of the present-day attitude towards hæmoptysis, influenza, pleurisy, and negative sputum examinations, etc.

XXXI

THAT COMPLETE IMMOBILISATION HAS NOT COME UP TO EXPECTATIONS

This is somewhat difficult to answer, as I had no definite expectations, but merely enunciated the fact that the fewer the movements of febrile, tuberculous patients, the sooner is the fall in temperature. And after twelve years' further experience, I am more than formerly convinced of the importance of immobilisation IF IT IS ATTAINED.

Let me illustrate what I mean. A Sister, in a very friendly way, made the statement contained in the above heading. I asked if she had any patient "on absolute," so that I could judge of her nursing. I was taken to see a girl lying in the true typhoid position; but she was rubbing her feet together, because she was cold!

The great difficulty is to make nurses appreciate the importance of minute details, especially *after* they are trained. I have never met one "trained" in the details of immobilisation by a general hospital. It should be self-evident that talking is muscular action. Every nurse is aware of the existence of the glossal muscles, as well as those of the thorax, yet it is difficult to make them realise its harmfulness. The number of nurses who are really acquainted with immobilisation must be extremely small. They certainly do not like learning about it, especially when they become

"Sisters." The last one I trained threatened to resign several times, but having survived, is now second to none in securing immobilisation.

Again, the temperature having fallen to normal, patients are too often permitted to do TOO much TOO soon—with a resulting return of fever. In other words, immobilisation is little comprehended, and still less is the art of management of the period between immobilisation and the time when a patient may be permitted to leave his bed.

XXXII

THAT "OPEN AIR" IS CURATIVE TREATMENT FOR TUBERCULOSIS

It is a pity the words "open air treatment" have become associated with tuberculosis. For one reason, it is not treatment; the air is the heritage of us all, and for the other, that even if we wished to do so, it is impossible in this country to live in the open air all the year round. Open air suggests living in the "open air," whereas the treatment by this so-called system is that patients spend part of their time in the open air and part in a room with the windows open. The real aim, however, is to see that patients pass all the time in pure air. The ambulant patients will be in the open most of the day, and at night will sleep in their room, or, if they prefer it, outside. I do not think it matters so long as there is a thorough

movement of air in the room, *i.e.*, that they have pure air. I should seldom place febrile, tuberculous patients out of doors in the daytime. They can have equally pure air in their wards, if these are properly designed. When kept indoors, they are much more immobile. The sun is not beloved by all febrile, tuberculous persons, and it usually tends to restlessness. I am convinced, therefore, that the control of the excessive auto-inoculation is much more effective when indoors. I certainly attach far greater importance to this control than to any benefit obtained by placing them in the sun. For the non-febrile patients and for children, and the world in general, the surest method of obtaining pure air is obviously to live always out of doors.

But in this country it is not possible; so when weather conditions are adverse, we must see to it that our habitations have a pure air supply. Much more attention will have to be paid to the necessity of securing a proper movement of air indoors. Leonard Hill[47] has placed this subject on a sound scientific basis.

In the old days, as is well known, it was the custom to confine persons suffering from tuberculosis in warm rooms, with closed windows, to prevent them "catching cold." As is equally well known, that method has given place to treatment with open windows. This *modus operandi* has changed from one using impure air to one of using ordinary, unvitiated air. Because, in the past, persons were foolish enough to live in bad air, we can hardly call it scientific treatment if

we live, as I presume we were meant always to live, in pure air. It follows that by placing persons suffering from tuberculosis in pure air, we are not carrying out any TREATMENT, but are merely advising them to live under natural conditions.

To put it in other words, it is not the pure air that does good, but the bad air that does harm, and it is certainly not a fair deduction that because impure air predisposes to disease, that pure air alone will cure it.

Take the analogy of a water supply. It has not yet been suggested that the health of a town is improved by a pure water supply, unless it supersedes an impure one which has already injured it. It is as necessary to have pure air as pure water. If, therefore, pure air were, of itself, a cure for tuberculosis, there would be no need for physicians, sanatoria, or tuberculin. Unfortunately, in addition to living in pure air, it is necessary to raise the specific resistance of the individual, and pure air alone will not, with certainty, accomplish this.

I have had practical experiences of treatment by impure and pure airs. When I first went to Brompton, in 1896, the hospital was ventilated on an artificial system, by which it was endeavoured to maintain, during the winter months, an even temperature of 65° F. or thereabouts. Night sweats were common and severe amongst the patients, and sore throats were frequent amongst the staff, both medical and nursing.

This system was replaced by that of open

windows in 1901, during the time I was Resident Medical Officer. The result has been that night sweats are now very uncommon, and practically never so severe, and sore throats are almost unknown. This is only one illustration of the benefits from the change.

The words "open air" have been applied to schools, whereas the phrase really means a "pure air" school. So that, here again, however much we should like to keep the children out of doors, it is impossible; they also must have a roof. Now it is just on cold, wet, winter days, when the children are forced to come in for shelter, that the real necessity for the supply of pure air arises. It is verily on such days that the schools are too often shut up, if IT CAN BE DONE. At Swansea I saw an excellent type of building; under no circumstances can it be closed; therefore, there is movement of air, and the children will indeed be treated on the pure air principle.

Let us consider this very important matter in practical details. It is obvious that pure air does not mean air that has already been breathed in the school, but air that has newly entered the building. This necessitates that the air should move right across the room at each respiration, so that if the room is 30 feet wide, and the respiration rate of the pupils 18 per minute, then the air should be moving at least at the rate of 540 feet per minute, or about 6 miles per hour, a fairly quick movement in a room. The children would only then, in theory, get ABSOLUTELY unbreathed air because all of them would now

respirate together. It, however, shows how these pure air problems must be considered for rainy days. Children under such conditions would not be as well off as in the open, but it would be a very close imitation. From a national health point of view it is to be regretted that all buildings used by children are not built on pure air principles; many still exist in which only a small percentage of the windows are made to open at all. It strikes at the foundations of health when growing children, the possible future harvest of tuberculosis, are being nurtured under what are not the best possible conditions. This is surely not the way to avoid a "C 3" population.

For a patient in a single ward, a movement of 3 feet of air per respiration should ensure a new supply of air at each breath. At a respirating rate of 18, this gives an air movement of 54 feet per minute, or less than a mile per hour. I give this to illustrate the fact that the smaller the wards of a tuberculosis institution the less the movement of air is felt.

The words "open air" have also caused the popular belief that a patient is better off in a hut in the garden than in a properly aired room. He is neither better off nor so comfortable. The hut for tuberculosis has its place where there is over-crowding, or where a person cannot have a properly ventilated room to himself. He can always sit or lie in the garden when the weather is propitious.

Open air "treatment" is still by many considered all that is necessary. There could be no

greater misconception. Pure air is better than impure air to all, whether tuberculous or not, but it will not, by itself, raise the resistance of the patient to his own toxins, which is the crucial matter to him.

XXXIII

THAT NIGHT AIR IS PREJUDICIAL TO TUBERCULOUS PATIENTS

It is curious how this idea arose. At one of the largest institutions for tuberculosis in France, the windows are widely open all day and closed at night. What is the difference between day air and night air? Even if it is lower in temperature than during the day, this would not explain the popular aversion, for the very cogent reason that air, even in frosty weather, is not considered deleterious but thoroughly wholesome.

The sooner these old-world ideas are subdued, and the windows of all bed-rooms are opened sufficiently wide to admit the pure health-supporting night air, the better it will be for the populace.

I often ponder on what air people think they are breathing during the night, if not the night air. They must breathe some sort of air. Pulling down the blinds does not prevent the "night air" entering. The net result to those who endeavour to avoid the night air is that they inhale a mixture of impure air left in the room from the daytime,

and the "night air" that enters through the crevices—the story of the ostrich once more.

XXXIV

THAT PINE TREES ARE NECESSARY IN THE TREATMENT OF TUBERCULOSIS

If tuberculosis were only a pulmonary disease and not, as it is, a general disease, the effects of pine-laden air might prove beneficial, as it is supposed to do in bronchitis. I have carried out the treatment of pulmonary tuberculosis both where there have been pine trees and where they have been absent, and I am satisfied that their presence or absence makes absolutely no difference to the progress of the patients. Indeed, at Llangwyfan, where they are conspicuous by their absence, the patients made remarkably good progress.

XXXV

THAT GREAT ALTITUDES ARE NECESSARY FOR THE TREATMENT OF TUBERCULOSIS

I have, as already stated, treated patients under sanatorium conditions in various parts of England and Wales. In no place have the patients made better, immediate progress than they did at

Llangwyfan, which is only very little above the sea-level at Rhyl.

Compare the elevated state of Colorado, with a death-rate from tuberculosis of 1·97 per 1000, with low-lying Norfolk and its rate of 00·74. Consider, also, the death-rate for Switzerland, 1·87 with that for flat Norfolk.

One Whitsuntide I happened to be staying near Geneva and some friends motored me to see a sanatorium in the French Alps. It was at a very high altitude, and in a most delightful situation. The owner was a medical man, who had contracted tuberculosis. At the time of my visit, he was in bed with pleurisy and a high temperature. The mountain air and altitude had not prevented him from an excessive auto-inoculation, nor did it control the inoculation when he was put to bed. We come back to the previous argument, that if altitude or anything else is going to cure, it should prevent quiescent tuberculosis becoming active.

XXXVI

THAT IT IS GOOD ADVICE TO TELL A MAN SUFFERING FROM TUBERCULOSIS TO CHANGE HIS OCCUPATION

From what I have previously written, it should be self-evident that this is wrong. Yet such advice is constantly being given.

In taking histories of patients admitted to

sanatoria, it is common to find that many have taken up the calling of insurance agent and similar jobs, because, having "weak lungs," they have been advised to give up the occupation to which they had been trained. The change of work, be it observed, does not prevent their eventually having to seek help from the sanatorium—too often too late. If a patient has inactive tuberculosis, to change his work means a fall in his market value, with the consequential fall in conditions of living and food values, both derogatory to his interests, and tending to cause activity of his disease. Whereas, if he has active tuberculosis he should have TREATMENT. Changing a man's work is not treatment.

After treatment, it is apparent that his prospects of retaining his health will be better if he abandons a bad occupation for a good one, always provided he suffers no serious decline in remuneration.

XXXVII

THAT A SPECIAL DIET IS NECESSARY IN THE TREATMENT OF PULMONARY TUBERCULOSIS

It is not the diet that matters so much, as the inducing of the patient to partake of it.

From the dietetic point of view, pulmonary tuberculosis may be divided into three classes :—

(1) The afebrile cases, with a fair or good resistance, which are making some sort of progress or, at any rate, are stationary ;

(2) The febrile cases which still are showing a fighting resistance; and

(3) Those cases whose resistance has gone: be they febrile or afebrile, they may live for weeks or years; but they are, all the same, dying men and women.

I can well remember the stuffing days with which the first class were treated, and it is quite common to hear people say that they are over. But are they? The moment tuberculosis is diagnosed, it comes to the tip of the tongue to order plenty of milk and butter, which are the fattening foods. So that the object in giving them is to make the patient fat. Why make the patient fat? Fatness does not increase the specific resistance in tuberculosis.

Obviously, if the patient is below normal weight, by all means let him have milk products. My point is, that we should endeavour to raise a patient up to his normal standard and keep him there, and not to endeavour to fatten him.

Fatness, caused by ingesting milk, connotes large deposits of fat in the omentum, with a prominent abdomen, and it becomes a hindrance to the diaphragm, thereby causing dyspnœa. It also means extra weight for the patient to carry when undergoing his exercise, and it may adversely increase his auto-inoculations.

These ideas have all crept in because the scales have been the criterion of progress in pulmonary tuberculosis. One could understand this when the disease was universally called and considered to be a "consuming" disease. Now it is known to be a

bacterial disease of the blood, and not a consuming disease, except in the latest stages, when all resistance has broken down. Therefore attention should be directed to the endeavour to try to increase the specific resistance of the patient to his own toxins. This, at the outset, is much more important than diet or weight.

The next point to remember is that, because a patient has tuberculosis, it does not prevent him from having any of the various forms of dyspepsia. The only gastric disturbances induced by tuberculosis are tuberculosis of the stomach, which is extremely rare, and anorexia, caused by the general toxæmia of the disease. No diet will improve this very much, until the cause is removed by increasing the patient's specific resistance to his disease. It follows, therefore, that if a patient with pulmonary tuberculosis has dyspepsia from any of the many causes but the two just mentioned, his diet should be the diet for that particular ailment, and it is not, therefore, a special diet for tuberculosis.

Since, therefore, tuberculosis is not a disease of nutrition, it is clear that diet is a comparatively simple, although an extremely important, problem. To put the matter in a nutshell: the diet should be that to which the patient has been accustomed in health, *plus* extra meat. If, however, the diet has obvious flaws, it should be adjusted so as to synchronise with the standard here given. This is the sort of diet that a London working man would adopt, if in receipt of ample wages. It must not be considered that it should remain the same week after week, but, rather, it should be

varied so as not to become monotonous. The habits of a community should also be studied. For instance, in some parts of the world, herrings and oatmeal are not so welcome as they are in Scotland, and all Scotsmen do not relish spaghetti. Pawlow's experiments show that the activity of the digestive glands is largely influenced by the antecedent mental attitude of the subject. A strong, instinctive desire for food is an essential factor in stimulating the digestive processes, and so maintains a progressive equilibrium between the secretion of the digestive fluids and the chemical reactions of the food ingested.

To give a practical example. Suppose it is desirable to induce the patients to eat much fat with their meat. This is very distasteful to some, but they will eat as much fat as is required if it is made into cakes or puddings. The aim, therefore, should be to arrange a dietary that will cause the patient to approach it with an eager zest, and not with any feeling of repugnance.

From this outlook, the importance of a dainty service is manifest. The best food can be spoilt from indifference in the manner of presenting it to the patient.

If the patient's resistance is good, he will probably have an excellent appetite, and this should be encouraged by dissuading him from partaking of nourishment between meals — not even a glass of milk should be allowed. The meals should be at regular intervals, so as to allow the stomach to absorb the nutriment, and have a rest before filling it again. If the appetite is poor,

forbid smoking for at least an hour before taking food.

The following table indicates a standard sanatorium diet :—

BREAKFASTS—8 A.M.

DAILY—Bread and butter, tea, coffee or cocoa, porridge and milk.

Sunday—Cold ham.
Monday—Bacon and egg.
Tuesday—Fried bacon.
Wednesday—Cold ham.
Thursday—Fried bacon.
Friday—Haddock, marmalade.
Saturday—Fried bacon.

DINNERS—1 P.M.

DAILY—Bread and potatoes.

Sunday—Cold salt silverside, salad or pickles, semolina pudding.
Monday—Roast beef, greens, rice pudding.
Tuesday—Roast lamb, turnips, apple pudding.
Wednesday—Roast pork, parsnips, cornflour and stewed fruit.
Thursday—Boiled mutton, onion sauce, greens, suet pudding and treacle.
Friday—Steak-and-kidney pudding, swedes, tapioca pudding.
Saturday—Roast veal, greens, roly-poly jam pudding.

SUPPERS—6.30 P.M.

DAILY—Bread and butter, tea or cocoa.

Sunday—Eggs, jam.
Monday—Cheese and salad.
Tuesday—Fried bloaters, jam.
Wednesday—Eggs, jam.
Thursday—Irish stew.
Friday—Cold boiled salt beef.
Saturday—Sausages and mashed potatoes.

These meals are only given as examples. In practice, there is never the same dinner on the same day of the week. This causes surprise, and prevents patients "feeding on their dinner beforehand," as they express it. Dishes which are included in the menu, but are not mentioned above, are stewed rabbit and pork, stewed steak, steak-and-kidney pie (hot or cold), veal-and-ham pie (hot or cold), soups of all kinds, fig puddings, cornflour and stewed fruits, apple dumplings, jam tarts, and fried potatoes.

Patients should be induced to eat heartily; but a minimum amount of fresh butcher's meat must be insisted upon. Personally, I consider that meat has some specific action in pulmonary tuberculosis, which may be due to the presence of vitamines. I found that the women in Wales ate meat with much greater zest and in greater quantities than did the women patients in England, and, certainly, in Wales they made better progress than the English. The meat-consuming capacity of the men was very much the same, as was also their improvement.

Fresh green vegetables do not find a sufficient place in the dietary in this country. They are valuable in preventing constipation, and they contain mineral salts and vitamines. Potatoes should always be cooked in their skins, so as to preserve the potash salts, which are mostly lost when they are peeled.

"Underdone" meat is both more digestible and more nutritious and contains more vitamines than that which is "well done."

All recent investigations have confirmed the importance of vitamines, and Leonard Hill is of opinion that the less food is handled or cooked the better. From this point of view, salads of all kinds, fresh fruits and nuts will make an additional attraction to the dietary. Conversely, tinned foods and fruits * suffer loss of vitamines in the process of canning.

W. E. Fitch,[46] in his work on *Dietotherapy*, classifies food in the treatment of tuberculosis into "allowed" and "forbidden." Under "forbidden" appear the following: "pork, veal, and all indigestible meats, stews, gravies, made dishes, fried potatoes, cucumbers, cabbages, parsnips, carrots, arrowroot cakes, fried foods, most sweets, pies and pastry." He gives no reasons for these prohibitions, and, as will be seen from the "standard" diet, the majority of the prohibited items appear in it. He advises the taking of food between meals, and that meals should not be more than three hours apart. He then adds: "indigestion dietary is often necessary." My experience has been that patients often begin treatment complaining of dyspepsia. Once they have been INDUCED to eat the diet here given, and to abandon the practice of taking nourishment between meals, the dyspepsia disappears.

The diet for the second class of patients referred to, should be the same as for the first—despite fever, hæmoptysis, or anything else. If the patient can swallow and retain this food, the chance of recovery is immensely greater than it

* Except tomatoes.

would be on a "light diet." There is a prejudice against ordering "butcher's" meat when a patient has fever. The more meat the febrile, tuberculous patient consumes, the more likely he is not to be himself consumed. Every possible endeavour should be made to keep to the standard diet. The patient's friends, from centuries of prejudice, will be against it. Once the change is made from full to light diet, the patient, in my opinion, has seriously lost ground.

It cannot be too strongly insisted on, once more, that the meals should be taken at regular intervals, and that the stomach, already at a disadvantage by reason of the toxæmia, should be given an opportunity for rest and recovery of tone. It is not possible for it to do its work if it is constantly harried. The enemies of the patient—I almost said friends—will urge that he should be given "something strengthening" between meals, such as beef-tea, etc. It is in this class that the importance of diet is paramount. The resistance has broken, and every effort must be made to recover it.

If the diet has to be changed, it is desirable to keep as near to it as possible, but chicken, game, rabbits, etc., may be substituted for meat.

If the patient cannot, or will not, take solid food, then recourse must be had to some of the many forms of liquid foods, such as milk and meat preparations.

The third class should be allowed to eat what they like. Nothing can be done to cure them, therefore place no restriction on their liberty or

their comfort. Permit them every licence, however much one may imagine their food will disagree with them. The Latin aphorism, "Mors minus pœnæ quam mora mortis habet," applies very well to these persons. Patients in this last stage can often eat anything with impunity. Even if there is persistent diarrhœa, I would leave the food to the patient's choice. The diarrhœa is, at this stage, practically always due to tuberculosis of the intestine, and, in my experience, diet has no influence on it. In any event, a patient may be trusted to abandon any fare which is followed by pain or discomfort in a measure out of proportion to the pleasure derived from it.

Alcohol is unnecessary in the great majority of cases, and need only be used for failing hearts or appetites.

After a patient has been discharged from treatment, the financial aspect must, in certain cases, enter into consideration. The following table, by Professor Thompson, shows what is cheap and what is dear, in the way of suitable foods. It gives the cost of food in Dublin in June 1914. The prices are not the same now, but it is probably a better guide than if the present values were taken.

The standard diet already given, was adopted after practical experience of the likes and dislikes of patients. I did not, in any way, consider its calorific value. I, however, include the calories here, as they are so given in Professor Thompson's table, and they are our present chemical standard of food values.

A physiological method of indicating the relationship between food on the chemist's table, and its value after entering the body, will be of more practical assistance. Take, for example, the fact that the pulses are recommended in lieu of meat, as having an equal value in tuberculosis

Amounts of Foodstuffs Purchasable for 1*d.*, 1914.

Food Substances.	Quantity for 1d.	Energy value in calories.	Cost in June 1914.
Bread . . .	$\frac{2}{3}$ lb.	827 calories	1½d. per lb.
Oatmeal . .	10 oz.	1162 „	1/10 per stone
Whole meal .	$11\frac{1}{5}$ „	1172 „	1/8 „
Indian meal .	1 lb.	1685 „	1/2 „
Rice . . .	$\frac{1}{2}$ „	815 „	2/4 „
Peas (dried) .	$\frac{1}{2}$ „	827 „	2d. per lb.
Potatoes . .	2 „	790 „	7d. per stone
Butter . .	1 oz.	225 „	1/4 per lb.
Margarine . .	$2\frac{2}{3}$ „	592 „	6d. „
Dripping . .	$2\frac{2}{3}$ „	767 „	6d. „
Cheese . .	$1\frac{3}{5}$ „	195 „	10d. „
Beef (medium fat)	$1\frac{7}{9}$ „	133 „	9d. „
Mutton . .	$1\frac{3}{5}$ „	131 „	10d. „
Bacon . . .	$1\frac{3}{5}$ „	260 „	10d. „
Herring . .	1 (only)	120 „	1d. each
Codfish . .	4 oz.	54 „	4d. per lb.
Eggs . . .	1 (only)	90 „	1d. each
Milk . . .	$\frac{2}{3}$ pint	135 „	1½d. per pint
Separated Milk .	$3\frac{1}{5}$ „	680 „	2½d. per gallon

and the merit of being purchasable at the rate of 827 calories (peas, dried) per penny, as compared with 133 calories (beef) for the same sum. That there is a difference not yet determined by chemical means, is obvious to all who compare the face of a butcher with that of a non-meat eater. This must not be considered a reflection, in any way, on those people who wrongly style themselves

vegetarians. It is merely given as an example of a fact.

All meats are expensive forms of food, but the price varies according to the cuts selected. The cheaper cuts, therefore, should be bought, as they are as nourishing as the more expensive joints. Frozen meat is also quite suitable, and should be purchased if it is lower in price. Butter is dear, and can be replaced by margarine and dripping; skimmed milk may also be substituted for the fresh article. Cheese is cheaper than meat, and American cheaper than English. The white fish, such as cod and haddock, contain less fat than the cheaper herring and mackerel, which are, therefore, more nourishing.

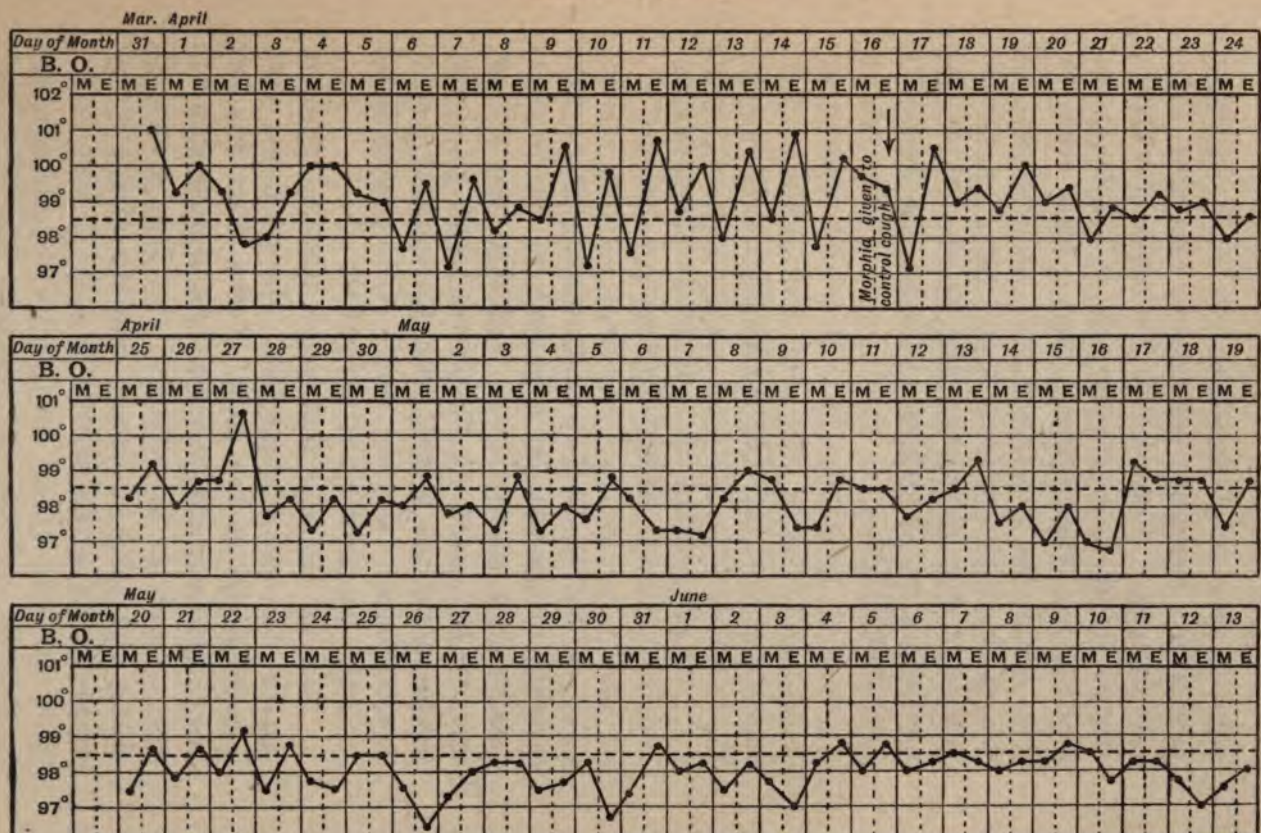
XXXVIII

THAT INHALATIONS ARE CURATIVE AGENTS IN TUBERCULOSIS

Creosote and other inhalations have their place in relieving the conditions which cause an irritable cough. If the patient is not troubled with this complication, then they are of little use. In fact, they may cause harm by diminishing the appetite. It is simple to explain, at least, one reason that has led observers to assume that inhalations are specific in character.

The accompanying Chart 15 well illustrates this point. It is a Chart of a febrile patient, in bed,

CHART 15.—Temperature Chart of a patient with an irritable cough. The fever does not settle down until the cough is controlled by morphia.



with a very troublesome cough. The exertion caused by coughing stimulates excessive auto-inoculations. Control of the auto-inoculation should result in a cessation of inoculations and a fall in the temperature. That is what occurred. The cough was suppressed by a sedative, and the fall of the temperature was not due to a specific action of the physic on the blood, but to the alleviation of the cough causing the excessive auto-inoculation.

XXXIX

THAT ASPIRIN IS A USEFUL DRUG IN TUBERCULOSIS

I do not think that aspirin, and similar anti-pyretic drugs, can be regarded in any way as specifics for tuberculosis. Certainly they do not remove the cause of the fever, which is, as has frequently been stated, due to excessive auto-inoculations. Control these auto-inoculations, and the temperature is controlled.

Let us suppose that after repeated doses of aspirin, the temperature falls. No good has been accomplished, as the cause of the fever has not been removed, and therefore the temperature will rise as soon as the effect of the drug wears off. From the point of view of appetite, the less such remedies are used the better.

XL

THAT COD-LIVER OIL IS NECESSARY IN THE TREATMENT OF TUBERCULOSIS

I have seen many hundreds of cases treated with cod-liver oil and also without it. I never employ it now, as a routine, as I consider it quite unnecessary. It certainly is no specific, and it may cause dyspepsia. The necessity of taking a full standard diet has been so laboured by me, that it goes without saying that anything which may even suggest an attack of dyspepsia is to be shunned.

The effect of cod-liver oil is to fatten patients; should this be necessary, it can, as a rule, be accomplished equally well, and much more pleasantly, by means of appropriate feeding. I am aware that this oil is beneficial in chronic bronchitis, asthma, and similar diseases, and where tuberculosis is thus complicated, its assistance may be necessary.

XLI

THAT EXERTION CAUSES HÆMOPTYSIS

I well remember, when starting the graduated labour scheme at the Brompton Hospital Sanatorium, how this old bogey used to haunt me. If this statement concerning hæmoptysis

were correct, it would have been impossible to introduce and maintain treatment by graduated labour, as the patients would refuse to undergo such treatment—if it were not manifestly to their advantage and not to their detriment. During the year 1909 the Brompton Sanatorium patients constructed a reservoir. In so doing, they cut down and removed sixty trees. They excavated and wheeled up hill, for a distance of 300 yards, 5000 tons of sand. They mixed by hand, and wheeled into position, 996 tons of concrete. The reservoir they constructed was 100 feet long, by 54 feet wide, and 14 feet deep, and held 500,000 gallons of water.

In the result not one case of hæmoptysis occurred whilst they were at work. The patients were is no way specially selected for that job, but were put to the construction as they came into the "grades" performing the task.

I think most ex-house physicians of Brompton will share my experience of our old hospital, namely, that the majority of cases of hæmoptysis happened at night and broke, too often, our well-earned rest. Patients suffered from hæmoptysis during the day, but then again, the patients were nearly always in bed. Very few cases occurred amongst the patients in the grounds. In Otis'[49] table of 18 cases, two-thirds took place at night. Bang[50] reports that of 354 cases of hæmoptysis, 69 per cent. happened whilst the patients were in bed or reclining in chairs. Tradition has it that such an accident occurred in the chapel at Brompton. Since that eventful day, a house

physician has had to attend each service on Sundays. But I never heard of his medical services being required.

Because patients have commenced an hæmoptysis whilst performing tasks, such as stooping to tie shoe-laces, it has been assumed that exercise has been the cause and not a coincidence. On the evidence given it would be equally reasonable to affirm that rest was a determining cause of hæmoptysis, and that, therefore, patients with tuberculosis should never go to bed if they wanted to avoid it, which is obviously absurd. The explanation may possibly be that when a patient is on hard work, with large quantities of blood in the muscular system, the pulmonary blood pressure is lower than when the muscles and body are resting. So far as I am aware, little or no work has been accomplished in comparing the pulmonary blood pressure during conditions of work and rest, so that there is no confirmation of this theory. There is, consequently, no scientific evidence that exercise causes hæmoptysis, and there is a great deal of evidence to show that it does not do so.

XLII

THAT SUCKING ICE IS GOOD AS AN ANTIDOTE FOR HÆMOPTYSIS

Ice applied direct to a bleeding surface does, as we are all aware, control hæmorrhage. In the

case of hæmoptysis, the blood is issuing from the lungs. The water from the melting ice goes to the stomach. The ice cannot have any effect on the bleeding point in the lung, unless the whole of the circulating blood is reduced in temperature by its action, which is, of course, impossible.

I consider the sucking of ice to be a bad practice; it entails exercise, keeps the patient restless, and has, in the end, no influence on the hæmoptysis. It is infinitely preferable to give the patient a hypodermic injection of morphia or heroin, which will ensure rest. Ice-bags on the chest are equally useless, unless they are used for a psychological effect (by inducing an idea in the patients' minds that something is being done for them) or to keep them still.

XLIII

THAT THE RESTRICTION OF FOOD AND LIQUIDS IS NECESSARY IN THE TREATMENT OF HÆMOPTYSIS

I have seen this treatment adopted for many years. I have also had similar experience of employing ordinary diet, and I am convinced that the former is bad, and the latter is good treatment. Apart from the depressing mental effect, and also the restlessness of the body due to thirst, the old method quite unnecessarily reduces the nutrition of the body, without having any apparently beneficial effect on the patient. I encourage the

patient to eat, and allow him anything he may request, in reason, my view being that diet alone has no influence on hæmoptysis.

XLIV

THAT COLD FOODS AND DRINKS ARE NECESSARY FOR THE TREATMENT OF HÆMOPTYSIS

I have also had experience of foods given either hot or cold, and if I myself suffered from hæmoptysis, I would certainly endeavour to have my food served hot. How hot or cold foods in the stomach can influence the bleeding vessel in the lung, I absolutely fail to see. They can, however, influence the comfort and nutrition of the patient, and want of comfort means too often restlessness, a condition of things, at all risks, to be avoided. Therefore give patients what they relish.

XLV

THAT ACUTE SEROUS TUBERCULOUS PLEURAL EFFUSIONS SHOULD BE ASPIRATED

As a medical student I was taught this doctrine, and afterwards blindly practised it for years. Had it not been for the principles of auto-inoculation enunciated by Wright,[52] I should, in all probability, be still following this very bad routine.

It is difficult, when the situation is analysed, to see any reason for withdrawing the fluid.

The procedure probably arose from a desire to relieve the dyspnœa. But the withdrawal of fluid does not relieve the distress. Whatever the cause of the dyspnœa, it certainly is not altogether due to the mechanical pressure of the fluid of the lung. If the fluid is left alone, and the patient treated by absolute rest, the acute symptoms will gradually disappear, and this long before the fluid lessens appreciably in quantity. These are facts recognised by few.

A patient at the onset of a tuberculous pneumothorax has distressing dyspnœa, that lasts for days or weeks, as is well known. It passes off usually in a few days or weeks, although the lung remains completely useless. It is too often overlooked that we have such a large reserve surface of lung for extra hard activities, that one lung can, even with fairly laborious exertion, keep the blood oxygenated. The case of the policeman, previously quoted, is an excellent illustration of this point; that there is nothing to be gained by aspiration.

The displacement of the heart by the fluid may also have had an influence on the performance of this operation. The heart may be out of place for months without any resultant harm. Why, therefore, should the fluid be withdrawn because the heart is transposed? The heart is often permanently displaced, in the long-standing cases of tuberculosis, by reason of contracting lung tissue, without any apparent ill effect.

The fluid may also have been withdrawn

because, being matter in the wrong place, it is considered to be harmful. On the contrary, it is extremely useful. We all realise that active tuberculosis requires the *utmost* attainable rest, to insure the *smallest* possible amount of movement of the affected organ. A tuberculous knee, therefore, we put in a splint. If it were possible always to place the active tuberculous lung in a splint, the treatment of tuberculosis would be much easier. When the natural processes come to our aid, and place the active lung in a splint, why take it away?

Probably the fluid is removed, in some cases, because it is thought dangerous to allow patients to leave their beds with a large amount of serous fluid in their chests. As soon as the temperature is normal, I have no hesitation in instructing patients gradually to get up, and provided the temperature remains normal, I have no qualms in starting them on induced auto-inoculations by exercise.

On the one hand, therefore, we have the mechanical aid of a splint to rest the lung; on the other, a pathological condition of the blood. The patient's resistance has temporarily broken down, and there has been a large invasion of the pleura. A fight is actively proceeding between bacilli and anti-bodies, and the production of additional anti-bodies is being stimulated. It is useful to remember Wright's words,[51] "Auto-inoculations may follow, or be induced, upon all active and passive movements which affect the focus of infection, and upon all vascular changes

which activate the lymph-stream in such a focus. . . . If the immunising response is adequate, the patient will recover." Here, then, we have the blood fluids in a very unstable condition. The "focus of infection" has undoubtedly been affected, and the "lymph-stream" has been heavily involved, as is evidenced by the effusion.

If, accordingly, we interfere with the pleural fluids, we "activate the lymph-stream in such a focus." How much, or how little, do we activate it? Who can tell? In this acute condition, if we "activate the lymph-stream," we may adversely upset the balance against the patient. Therefore to touch the fluid is precarious for the patient. There is definite evidence [52] that the blood pressure is influenced by aspiration, which indicates that such an operation has an influence all over the body.

On the other side of the picture, nearly all these cases of acute pleural effusions recover of themselves in about three weeks, if left alone, and kept on absolute rest. Both these points are equally important.

To sum up: Never interfere with fluid in an acute tuberculous pleural inflammation, even when associated with severe dyspnœa and much displacement of the heart, until all acute symptoms have disappeared, unless driven to do so. The affected side is for the time being the acute side, consequently the acute lung is at rest, and the temporary extra strain thrown on the other organ will not harm it, as the one where the disease is active is under ideal conditions.

As soon as the patient is able to be up and about, with a normal temperature, the danger is past. Where there is still much fluid and dyspnœa, I have not seen any harm follow small withdrawals —up to about a pint-and-a-half. I should never run the risk of activating the disease by removing it all. And it is unnecessary to do so, as these cases almost invariably clear up, and the patient is usually better after the pleurisy if treated on the lines indicated.

Finally, by withdrawal of fluid, I do not, obviously, refer to the taking of a small sample for diagnostic purposes. I have no recollection of having harmful results from this procedure. If there is any doubt about the diagnosis of a pleurisy, in every case a guinea-pig should be inoculated with the fluid. A positive result here, manifestly, means not only infection but active disease. It is an immense advantage to the patient to know that he has tuberculosis, instead of waiting a year or two to reach such a conclusion, and so losing precious opportunities of treatment. There is also to be considered the gain of having accurate diagnosis and ascertaining definitely what is the source of the pleurisy.

"Pleurisy" is not itself a diagnosis unless the cause is known, nor is pleural effusion, unless, also, the origin is recognised. We are, really, here as with the lung, concerned with a general disease with a local manifestation. The word "idiopathic" used in connection with pleurisy can only be comparable with the "P.U.O." used in the Army, and therefore must indicate "pleurisy of unknown origin."

Pleurisy is not itself a disease but the resultant condition following the action of microbes or violence.

XLVI

THAT IT IS GOOD PRACTICE TO OPEN A TUBERCULOUS EMPYEMA

It may be well to do so, but I have seen too many such operations at Brompton Hospital ever to be induced to submit to draining tuberculous pus from *my* chest, if it became necessary to consider such an operation. I have never known one of these discharging tuberculous wounds to heal. I understand too well what it entails. It means that one has to pass through life with a poultice of smelling pus covering one side of the chest. I should be well content to retain the pus in my thorax, and have it aspirated when necessary. Probably I should not live so long, but I should be far happier. It follows, of course, that I would not entertain the idea of an Estlander's operation in a case due to tuberculosis.

XLVII

THAT IT IS GOOD PRACTICE TO SCRAPE A TUBERCULOUS LARYNX

The practice of interfering with the larynx is, I believe, falling fast into disrepute, owing to the disasters that sometimes follow such action. The danger lies in "activating the lymph- and blood-streams," and so causing an excessive auto-inoculation. I have seen this follow both cauterisation and painting with lactic acid, although at the time I did not understand the phenomena following the application of this form of treatment.

I am aware that there are a very few occasions when operative interference is imperative, because of the dyspnœa occasioned by the occlusion of the walls of the larynx, and that tracheotomy has to be performed. Here the wound is below the affected zone, and there is not the same risk of an excessive auto-inoculation, and it ensures rest to the affected part, which has proved to be the best of all treatments. As has been stated, I have observed the operative treatment, and I have also seen perfect results follow vocal rest. If my own larynx were involved, nothing would induce me to have it touched, unless the necessity arose from stenosis.

XLVIII

THAT GOING TO BOURNEMOUTH OR TO THE SEA-SIDE IS GOOD TREATMENT FOR TUBERCULOSIS

Whilst I was writing this book the following document came into my hands, and I reproduce it here solely for that reason. Many such are written, and I have read dozens in similar terms.

"I certify that this day I have examined A.B.C.D. I cannot find any physical signs of disease in his chest. I am, however, anxious as to his condition because of the cough, sputum and hæmoptysis. It is essential for him to have a complete rest and change, and I therefore advise that he should spend a month at Bournemouth. There is every reason to hope that his health will be restored by this change of air."

On reading the above, it is self-evident that there is little doubt in the writer's mind that the patient has active tuberculosis. The physician was so sure of the patient's ailment that he did not have the sputum examined. I heard details of the patient some weeks after the above was written.

He gave a history of continuous loss of weight, cough, and morning sputum for some six or seven months, together with an increasing feeling of tiredness and fatigue after work. He appeared to be in good health. He had definite signs at one apex, and tubercle bacilli were found in his sputum. A real case of early tuberculosis, with the resist-

ance commencing to give, but the man is still master of his disease. If we could only reach every case at this stage, tuberculosis would be a much easier problem.

To digress for a moment. Patients infected by the tubercle bacillus may be divided into the four following classes :—

(1) Those who recover without knowledge of their infection ;

(2) Those who recover after a holiday in the country or at the sea-side ;

(3) Those who require to have their specific resistance raised by methods additional to those of general hygiene ; and

(4) Those who are hopeless from the onset.

Class 1 does not concern us.

Classes 2 and 3 are the important ones. Have we any method of ascertaining by means of examinations of the chest, etc., spread over a week or two, to which class a patient belongs? We have not, and until we possess this knowledge it is better for the patient to consider him potentially active until we have ascertained that he is not. If he resides at the sea-side for a month he may recover his health. But why, in tuberculosis, run this risk of "may," when there is a better and more certain method?

Why, in tuberculosis alone, do we take these risks? Surely it is wiser to use all possible curative steps, as we do in other diseases. Apparently, it is not sufficiently realised that a man may look well and yet have commenced to die. It is impossible to foresee what the end of a case of

tuberculosis will be, in its early days, when the resistance is diminishing. If I had tuberculosis, I would not consider advice to visit the sea-side as sound counsel. If a man has tuberculosis, and has to abandon work for a month or six weeks, surely the wisest course would be to send him to a sanatorium, where he could have all the supposed advantages of Bournemouth. In addition, he could have the enormous benefit of treatment if he went to a sanatorium, and not to a home for tuberculosis. He would have the great advantage over Bournemouth in that he would, in addition, learn how to live. I do not think this latter point is sufficiently realised by the medical profession.

A month or six weeks is a very short period of time for sanatorium treatment, and the writer of such a statement should send a certificate to the head of the institution in the following terms :—

"I certify that A.B.C.D. is suffering from pulmonary tuberculosis to such a slight degree that I am of opinion that one month's treatment would be sufficient. I consider that he is, so far as his disease is concerned, fit for the hardest work—that is, the final grade of treatment by graduated labour, and I will take the responsibility should the hard work cause pleurisy and so delay his treatment for several months."

If he is cured, he must be able to stand the test exercises.

How many medical men would care to sign such a certificate? Yet it is the only logical sequence of the document quoted above.

If such a case does arrive at a sanatorium, the

relatives at once desire to have an opinion as to his probable progress. They usually volunteer the information that there is very little wrong with the patient, and they anticipate a rosy prognosis. I invariably reply, that if I had tuberculosis, I should be very seriously distressed, even if I thought I had an excellent prospect of recovery. It is not sufficiently realised that every advanced and dying case was once in this position.

If Bournemouth has a climate which cures persons suffering from tuberculosis, surely it should perform one of the following miracles :—

(1) Either it should kill all the bacilli in Bournemouth ;

(2) Or cure each individual immediately after infection, *i.e.*, it should cure "early" cases.

It should, consequently, result that the incidence of tuberculosis in Bournemouth would be extremely low, or negligible.

The best information on this point can, I think, be obtained from studying the mortality rates for Bournemouth, as compared with other places.

The average rates given here are for the period 1911-14. They are not given as before 1911 because the rates were not previously corrected, and not since 1914 because it is a controversial matter as to how far the war has affected normal conditions.

In order that there may be no misunderstanding, "corrected rates" means that persons belonging to London, or any other place, going to Bournemouth and dying there from tuberculosis,

are returned as deaths in London, if London is their permanent residence.

Mortality from Pulmonary Tuberculosis, all Ages.[61]

Place.	Per million living.	Per 1000.
Bournemouth	1165	1·16
England and Wales . .	1068	1·06
Grimsby	895	0·89
Blackburn	872	0·87
Dudley	764	0·76

Bournemouth is a borough, and I have considered only the statistics of the other boroughs. Bournemouth air apparently does less than Dudley, in the so-called Black Country, to prevent her inhabitants dying from tuberculosis. It would obviously be absurd to order anyone to go to Dudley for a month's rest because he had tuberculosis, and yet from these facts, as far as climate is concerned, it would appear to be the wiser course. This must not be considered in any way a reflection on Bournemouth. I have spent many happy days there. I simply wish to affirm that climate has little, if anything, to do with the treatment of tuberculosis. From the table given elsewhere, showing the death-rates in other countries, Bournemouth is in good company, in so far as the inability of its air to control a blood disease is concerned.

What, then, are the reasons for writing certificates such as that quoted above? Yet, unfortunately, many such are written.

There are some who consider the proposal to send all persons suffering from tuberculosis to sanatoria too great a financial proposition, and that the end does not justify the means. As usual, the difficulties are due to want of knowledge.

Firstly, every person infected with tuberculosis may not necessarily require sanatorium treatment, as for example, the Austrian soldiers, who gave a tuberculin reaction. Tuberculosis had infected them, but they were in no way in need of treatment, because their resistance had not broken down. Individuals may have acquired tuberculosis, as is evidenced by bacilli in their sputum, and yet be in perfect health. There is no necessity for treatment in these cases. Only those patients, therefore, who furnish evidence of activity require attention. The slightest indication of the breaking of resistance should denote immediate and skilled treatment.

It is advanced in support of other arguments, that it is unfair to condemn a person to the stigma of tuberculosis, and to a long period of treatment in a sanatorium, on such slight grounds. I often wonder, when hearing such remarks, whether it is realised that failure to "condemn a man to three or four months' sanatorium treatment" is actually, in too many instances, condemning him to death or permanent invalidity?

The alternatives to proper treatment are two. One: the infected person may recover without any treatment. Nobody can deny this, nor can one foretell whether the patient will or will not regain his health. He "may" — that is all that can

be said. The other: that the sufferer will develop further tuberculosis; however slightly this is so, he becomes a more advanced case. He is, therefore, going to take longer to treat, longer to repair, and to cost more for treatment, and, finally, his opportunity for regaining his health may have passed.

Any patient who, having undergone sanatorium treatment, takes a retrospective view of the situation, will be glad that he was dealt with in the earliest stages of the disease, and at the same time taught how to live, rather than that any risks should have been taken with his life. He will even be thankful he was "condemned to sanatorium treatment." I repeat that it is never thoroughly understood that nearly every case of pulmonary tuberculosis has this critical moment—the moment when the resistance is giving, and when a very little assistance will prevent the "give," and render a permanent repair reasonably probable.

XLIX

THAT A SEA VOYAGE IS GOOD TREATMENT FOR TUBERCULOSIS

The advice to take a sea voyage is so frequently given, that it cannot be fully realised that the ships of to-day are not the sailing vessels of yesterday. Going to sea is too often associated with the novels of Captain Marryat, rather than with the

actual prevailing conditions. In his days, all seamen kept their watch ON deck, and when the necessity arose, went aloft, and lying on their bellies on a yard, singing appropriate chanties, clewed up the bunt of the fore-topgallant-sail, or performed similar tasks. If, to-day, a sea voyage meant such a life, it certainly would have the advantages of healthful work in the open air. But the wind-jammers are rare to-day, and the great majority of seamen are employed BELOW and NOT on deck. I once overheard this fact very forcibly put by a seaman, who was being interrogated, after his first voyage, as to his liking for the sea—"Lor' lum' me, the only blinking time as I sees salt water is when I blinking well pulls the plug of the W.C.!"

It will be a surprise to many to learn that even the quarter-master at the wheel is often completely shut up in the chart-room and does not see the sea. Only the watch-keeping officers, a few look-out men, and the hands who clean the decks, are employed in the open.

Passenger ships are designed to carry the maximum number of passengers, and not for the benefit of persons suffering from tuberculosis. They consist, therefore, of tier upon tier of cabins, being much like a hotel, with the disadvantage that they have extremely few, and small, windows, or ports, which frequently have to be kept tightly closed. He who aspires to a deck-cabin, that is, as it were, one on the roof, must have a well-filled purse. The cabins usually hold from two to four passengers, in a cubic space that on shore would

be condemned by the health authorities as being insanitary from over-crowding. Some ships have systems of artificial ventilation. It is, however, unnecessary to go to sea to live in, what the Navy very aptly term, "potted air."

In addition to the lack of pure air, the opportunity for exercise is small. This is clearly demonstrated by the fact that some shipping companies are providing on their vessels gymnasiums and swimming-baths; but neither of these are on deck and must be artificially ventilated. Theoretically, one should live all the time in the open air at sea, but in practice one does not.

Consider also the case of a young man sent to sea for his health. Having nothing to do, he will spend too much time in the smoking-room and bar, and will learn too often both to drink too much and to gamble. Whereas he ought to be under the strictest supervision of a medical man, and be treated and tested by graded exercise, interposed with rest, together with regular hours, sufficient sleep, and his whole time should be spent in pure air.

Conversely, it is very unfair to send a tuberculous person to sea to sleep with healthy persons in an ill-ventilated cabin. More especially is this so, as the patient will conceal his ailment, for obvious reasons. This is from the passengers' side of a sea voyage.

Let us next consider a few statistics concerning nautical workers. In the last Report of the Director-General of the Navy,[58] it is stated that the average number of cases of tuberculosis (all

forms) occurring in the Service for the quinquennial period ending 1913, was 2·59 per 1000 of strength, and the following table[54] indicates the length of service of the men invalided from the Navy during 1913 as suffering from that disease.

	Under 1 year.	Under 2 years.	Under 3 years.	Under 4 years.	Under 5 years.	5 to 10 years.	10 to 15 years.	15 to 20 years.	20 years and upwards.
Men. .	19	14	23	13	19	53	44	29	7
Per cent. .	8·6	6·3	10·4	5·8	8·5	23·9	19·9	13·1	3·1

This table is good evidence that the disease was either contracted in the Service, or that the conditions of the sea-life were favourable to the lighting-up of an old lesion. If, therefore, the sea is going to cure tuberculosis, it ought to prevent men from acquiring tuberculosis. If it does not do so, it should cure them before they are aware of their illness, *i.e.*, when the infection took place and they were really "early" cases of tuberculosis. Of all the deaths from disease for the quinquennial period ending 1913 in the Navy,[55] one in every 8·23 deaths was caused by tuberculosis. Tuberculosis was the third highest causation of death by disease, and also the third highest cause of invalidism.

It is necessary to emphasise that the invalidity and deaths due to tuberculosis occurred not amongst the "C 3" population, but in the pick of the country. All the men had to pass a searching medical and physical examination, and therefore were all apparently in perfect health and condition

when they entered the Service, and were well fed afterwards. Yet in spite of "going to sea," tuberculosis has undermined their health.

The volunteers for the Japanese Navy,[56] numbering 23,453, were medically examined, and only 35·83 per cent. were accepted during 1911, so that their standard must also be a high one. Nevertheless, the annual average of new cases of tuberculosis for the decennial period ending 1910 was 6·78 per 1000 of strength.

Surgeon Thompson[82] makes the following observations on tuberculosis in the Navy of the United States:—

"No person enters the Navy except when found qualified by a searching physical examination. The rejections far outnumber those passed in. It is a fact that many are called but few are chosen. The personnel is hand-picked, physically. The single rule that there must be a specific weight for a certain height, probably causes the rejection of a great number of men with incipient and unrecognised tuberculosis."

During 1914 the case incidence of tuberculosis was 7·2 per 1000 of strength. He continues: "Yet in spite of this enforced selection, that should place the Navy in a high expectancy class, the fact that so many cases of tuberculosis occur is alarming. It means that most of these men are infected after their entrance into the Navy. It means that the disease is acquired in spite of the precautions, rules and regulations which are in force and practised to prevent it."

There are no published statistics concerning the

death-rate from tuberculosis amongst the magnificent men of that great Service, the Mercantile Marine, under whose flag I have sailed the seas as a "seaman."

No information is available, concerning the matter under review, from the mortality statistics of either Scotland or Ireland, but the deaths in England and Wales are known. It would seem therefore, at first sight, that to obtain a mortality rate would be a fairly easy matter. The reverse, however, is the case, for the obvious reason that all sailors are not in home ports at the time of the census. So that, although the number of deaths is known, the population amongst whom they occur, is to the Registrar-General of England, an unknown quantity.

The Registrar-General of Seamen[81] for the British Islands writes on the census of 1911 as follows: "What is the total number of seamen, in the different ratings, actually following the sea as a means of livelihood? There are no statistics enabling a definite reply to be given to the question, but with a view to form some concrete idea on the number, it is perhaps desirable to make an attempt at an approximation." He arrives at the figure 147,530 as the number for the British Islands, that is, England, Wales, Scotland, Ireland, the Channel Islands, and the Isle of Man. The number of seamen belonging separately to each of these units is not known.

In examining the following statistics, remember that all persons who form a ship's crew, including the stewardesses, are classed as seamen by the

Board of Trade. The 1911 census disclosed that there were 21,645 seamen in Scotland at the time. Of these, one-fourth were English, Welsh, Irish, or foreigners, the actual number of Scottish mariners being 16,380. The count of seafarers in Ireland was 7441. The score of foreigners is given, but not the poll for the English, Welsh, or Scots. So to obtain a comparable figure it is necessary to subtract one quarter. The result is 5580. If, therefore, 16,380 + 5580 is deducted from the total of sea-workers, it will give, approximately, the number of seamen in England and Wales, and the result is 125,570.

The Registrar-General of England and Wales has given me the numbers of deaths of seamen from tuberculosis during 1910-11-12, and the average for the three years is 362. If there is this number of deaths in a population of 125,570, the death-rate is 2·98 per 1000 of those who go down to the sea in ships. The death-rate from tuberculosis of all males in England and Wales above 15 years of age, who do not hearken unto the call of the sea, is only 1·66 per 1000.[88]

It is, therefore, clearly wrong to send a tuberculous man to sea in any working capacity. On the other hand, to advise a tuberculous person to go for a sea voyage, to live on "potted air," is not recommending the best available treatment.

The configuration of the coast-line of Scotland is one which provides many lochs and inland waters, on which it is possible for numbers of the Scottish fishermen to ply their calling in open boats. The more exposed coast of England

makes it necessary to use ships which are decked in. From the point of view, therefore, of open-air occupation, the Scottish fishermen are exceptionally fortunate. Consequently, if an open-air life at sea is going to cure tuberculosis, the Scottish fishermen should be almost entirely free from that disease.

The deaths from disease amongst all males between the ages 25-65 in Scotland[84] during the years 1900-2 were 36,880, whilst the deaths from tuberculosis were 4005, in the same class and years, so that 19·29 per cent. of all deaths from disease were due to tuberculosis in all Scotsmen.

During the same years, the number of fishermen who died from disease between 25-65 was 549, and of these 147 succumbed to tuberculosis.

It thus happens that in fishermen, of all the men dying of disease, 26·70 per cent. have their lives ended by tuberculosis, whilst amongst all Scotsmen the percentage of tuberculosis mortality to all other deaths by disease is only 19·29.

Let me repeat. If a "sea voyage" is a cure for tuberculosis, it should either kill all the bacilli at sea, or should cure all the workers on boats and ships, just after infection—that is, when they are "early" cases.

The evidence is all against a sea voyage doing either.

L

THAT EMIGRATION, *i.e.*, A CHANGE OF CLIMATE, IS GOOD FOR TUBERCULOSIS

Recently the question was asked in Parliament if grants would not be made to tuberculous ex-soldiers, who wished to emigrate, and the reply was, that such assistance would be given, on MEDICAL GROUNDS, to those cases which would benefit by emigration. This, naturally, means emigration is still considered by some to have medical benefits.

Emigration, of course, signifies that climate is to be used in the treatment of the disease. That climate will beneficially affect a lung disease such as asthma or chronic bronchitis, most medical men will admit. But pulmonary tuberculosis is not a lung disease—it is a blood disease. So it is necessary to examine the case for climatic benefit from the point of view of its effects on the blood, and not on the lungs.

The countries usually recommended for tuberculous emigrants are America, parts of Canada, Australia, South Africa, and Egypt. If the climate of any of these countries has a curative effect upon pulmonary tuberculosis, one would expect to find that, at least, the climate would prevent tuberculous disease in these places. In other words, it should:

(1) Either exterminate the bacilli;

(2) Or cure the case immediately after infection

—that is when it is an "early" case and the cure easy.

There should, therefore, be practically no tuberculosis in these countries, yet as a matter of fact tuberculosis is prevalent in them all, as will be seen from the accompanying table, and the respective local authorities have just the same problems to face as ourselves, in spite of differences of a climatic nature.

Table [58, 59, 60] *showing the Mortality Rates from Pulmonary Tuberculosis per* 1000 *persons living, for the quinquennial period* 1901-1905, *in the following countries, as compared with England and Wales:—*

Ireland . .	2·15	Ontario (Province of)	1·29
Switzerland .	1·87	England and Wales.	1·22
Durban, S.A. .	1·73	Australia . .	0·85
United States .	1·7	New Zealand . .	0·7
Scotland . .	1·46	Tasmania . .	0·63

The following table shows the death-rates of some of the American States. Note the high death-rates of the two places chiefly recommended for tuberculosis, Colorado and California. These are possibly uncorrected rates, in which case it illustrates that the persons seeking health from those climates have failed to find it.

The vital statistics concerning tuberculosis are not very satisfactory for comparison. For instance, there are none for all the States of America; each State being a legislative unit unto itself. In Canada, Ontario is the only province which has material available. It has, however, one of the best climates in Canada. In South Africa, there are no statistics for *all* the deaths, except in a

few places, such as Durban. Here, in 1914, there were 501 deaths amongst the WHITE population alone, being 5·89 per cent. of the deaths from all causes.

The statistics are, however, sufficient for my purpose, for they clearly show that tuberculosis is prevalent in all the countries which are recommended as health resorts. Each country named

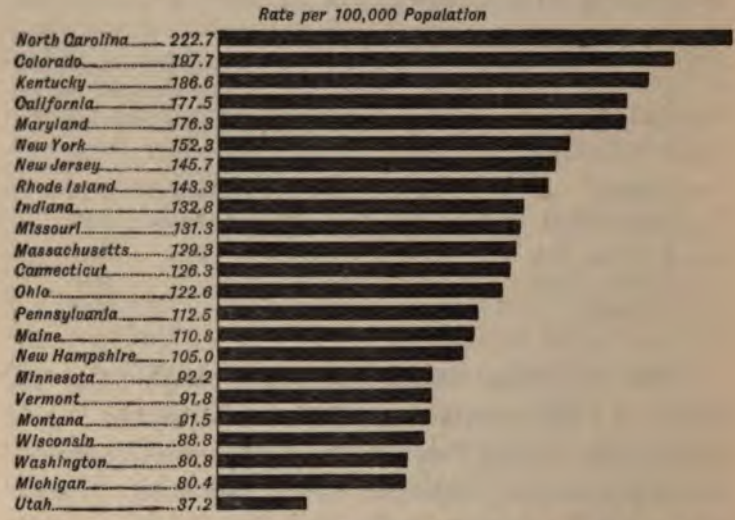


CHART 16.—Tabulation by The Prudential Insurance Co. of America.

is trying to face its own tuberculosis problems, and is not trusting to its climate to perform the work.

As has been written for the years 1911-12-13-14, the average mortality rate for tuberculosis at Dudley [61] was ·76, as compared with the above table. With three exceptions, Dudley, in the potteries, therefore has apparently the greatest claim to an anti-tuberculosis climate.

So many people have unsuccessfully tried the climatic cure in these over-seas countries that anyone detected with tuberculosis is not allowed to land in New Zealand, Australia, Canada or the United States; and from the United States and Canada immigrants are deported if tuberculosis becomes evident within two years of their landing.

I have treated tuberculous patients in London, Frimley, Devonshire, West Wales, and in the Vale of Clwyd—in wet, dry, and bracing climates—and I say without hesitation that all a tuberculous person requires in the way of climate is a place where the air is not relaxing, and this can be obtained in many parts of England.

In the treatment of tuberculosis, the manner in which a sanatorium is conducted is of paramount importance. If I had to choose, so far as climate is concerned, between a home for tuberculosis in Colorado, where the treatment was on the lines of "doing a little gardening," and a sanatorium in Whitechapel conducted by a man versed in control of auto-inoculation, and who had the other necessary qualifications, *i.e.*, a temperamental understanding of men and women, I should not hesitate to prefer Whitechapel.

Before the theory of auto-inoculation was understood, climate might have been considered. But now that we realise that successful treatment depends on control of toxins, it is difficult to understand why climate or place should control auto-inoculation. The most marked evidence against the lung theory one has experienced, was the behaviour of the patients at Brompton

Hospital during the days of the thick London fogs. Those who had chronic bronchitis, emphysema, etc., coughed in a most distressing manner, whilst those with tuberculosis were practically unaffected.

Emigration has many serious drawbacks. The only valid reason for emigrating is the certainty of obtaining work which is not obtainable at home. Inadequate means and indefinite prospects are not a suitable equipment for a tuberculous emigrant. The popular notion that all places outside England are health resorts is erroneous. Medical men in America, Canada, and Australia, have often expressed to me their astonishment at the haphazard and indiscriminate way in which tuberculous persons are advised to leave their country of origin. If an ex-patient has friends to go to, if he has definite work at an adequate wage, there is no objection to his emigrating. He must, however, take into consideration his wage-earning capacity in relation to the cost of living in the country to which he goes.

LI

THAT A WARM CLIMATE IS GOOD FOR TUBERCULOSIS

I can best illustrate the fallacy of this idea by citing a particular case in which I was interested in May 1919.

A lady music-teacher was ill in bed for several

weeks, suffering from "influenza." After she had been up a week or so, her medical attendant one day informed her that her lungs were affected, and that she must give up all work and proceed at once to the warm climate of Egypt.

Let us again apply the test to this counsel: "If the country or climate cures the disease, it certainly must prevent the disease being prevalent." The climate, or air, or whatever it is, therefore, should do one of two things—either kill all the tubercle bacilli in the country, or cure all persons immediately after their infection, so that they practically are never aware of the disease. This, of course, means that if climatic influences are paramount, they ought to cure all cases of tuberculosis at their very commencement, *i.e.*, when they are "early" cases. I am quite aware this has already been said, but it bears repetition.

P. Tribe,[62] of the Kasr-el-Aini Hospital, writing in 1904, states that pulmonary tuberculosis is a fairly common disease in Egypt; that when "other casual factors are favourable to the development of phthisis, even such perfect climatic conditions as Egypt possesses are powerless to prevent its occurrence, or to promote, unaided, its cure."

For the quinquennial period ending 1912, the admissions of soldiers suffering from pulmonary tuberculosis to Army hospitals[76] was 1·7 per 1000 of strength in the United Kingdom, whilst the rate for those serving in Egypt was 1·6. The death-rates per 1000 of strength being ·17 at home and ·26 in Egypt. The average number of

soldiers constantly sick in Great Britain was ·21, as compared with ·35 in Egypt. All the figures refer to tuberculosis. All men had passed a rigid medical examination. All men would be apparently in good health on landing. They all, therefore, probably started in Egypt as "early" cases.

During the year of war, 1916, 644 soldiers were admitted from the Army in Egypt[63] to hospital suffering from tuberculosis. These men had all previously passed a medical examination. Very few of them could have been actually ill when they landed. Consequently they must, in nearly every instance, have started in Egypt as "early" cases. In addition to this fact, they presumably had been living the open-air life in Egypt, *i.e.*, in tents, yet they had lost their resistance to the disease and had to "go sick" in that climate, which is very good evidence that it certainly does not cure "early" cases. Therefore it is less likely still to have any curative effect on more advanced conditions.

Apparently, tuberculosis is not yet a notifiable disease in Egypt. There are not, so far as I can ascertain from the Annual Report of the Department of Health, any available vital statistics concerning tuberculosis. These are, however, available for the "warm climate" of the Straits Settlements.[64] They are as follows: With a population of 759,067 they had, in 1914, 2097 deaths from tuberculosis. This is a death-rate of 2·74 per 1000 living—a greater mortality than that of cooler Scotland, where the rate was 1·62[65]

in the same year. The mean temperature of the Straits Settlements was 83·2° F. in 1914, the average temperature of Scotland being 47·4° F. in the corresponding period.

The case rate of tuberculosis in the Navy[66] for the quinquennial period ending 1912 was 4·42 per 1000 of strength on the East Indies Station, as compared with the rate of 2·27 of the Home Fleet.

Theodore Williams[67] stated at the Sixth International Congress: "My statistics demonstrated that even in Madeira, where the warm climate allowed of a life in the open for most of the twenty-four hours, the patients did not prosper but lost appetite, became languid, taking only passive exercise in a hammock. . . . The only ones who flourished were those patients who took cod-liver oil steadily and were strong enough to ride on horseback about the island." So that here it was, in spite of the climate, that some patients got well. They took exercise and improved in health — that is, they induced auto-inoculations and recovered, whilst those resting made no progress. It is a capital illustration of my point, that control of auto-inoculation is more important than climate. Patients are better off in a properly conducted sanatorium in England than they are in trying climatic "cures."

So much for the climate. Now consider the time of year. However wonderful may be the climate of Egypt in the winter months, it certainly would not appeal to many invalids during the hot season, and a patient with tuberculosis would find

it very uncomfortable to spend the warmest months of the year in Egypt. Next comes the question, where is the patient to go when she arrives in Egypt? Naturally, to some hotel or boarding-house. This is not good treatment for tuberculosis. What kind of a welcome is she likely to receive when her disease becomes known? Both these practical details must be considered before giving such advice. Then there is a fortnight's sea voyage, probably in a stuffy cabin, not to mention sea-sickness, which is not helpful even from the point of view of nutrition. The risk to the other passengers using the same cabin must also be considered, for the reason that they will not be pleasant to her if she uses a sputum flask; she must therefore conceal her ailment.

At the end of it all, what is the position? The lady will spend a great deal of money, and she *may* recover. It has already been stated that persons regain their health by change of scene, but that there is no means of ascertaining beforehand which patients require treatment and which do not. On the other hand, she has gone to the land of strangers and may quite easily, by over-exertion, destroy her chance of recovery by receiving an excessive auto-inoculation, which even the climate of Egypt can do nothing to control.

LII

THAT FARM COLONIES ARE NECESSARY FOR THE AFTER-CARE OF TUBERCULOUS PERSONS*

The only point in this saying with which I agree are the words "after-care." Some of the present-day advocates of these colonies are mixing up two things which should be kept apart—treatment and training. In the case of all other diseases, it is usual to treat the patient until he has recovered, and is fit for work. Then, if necessary, his economic position is considered. It is not customary to intermingle treatment and strategy.

To-day, certain patients are being taught new methods of earning a living whilst under some sort of treatment. This is an entirely wrong procedure, both medically and financially, as I shall demonstrate. I have no desire to disparage any work that is now being undertaken. My endeavour is rather to review the whole position from the horizon of the patients and of the State.

Whatever steps may be taken in the direction of colonisation, will not be completed without, I hope, due consideration of bed-rock facts and not theories. I have spent practically all my professional life amongst tuberculous patients, and I think I know their innermost thoughts.

It is necessary first to clear the air by eliminating the word "farm." To begin with, it is not a

* The principles of this article were communicated to the Ministry of Pensions in August 1917. The detailed scheme was written in April 1919.

sine quâ non for all patients to change their occupation; therefore it is unnecessary for all of them to live on a farm. There are a certain number of persons, probably a fairly large percentage, who after the age of twenty-five or thereabouts, either will not or cannot learn nature's ways. I should estimate, from my knowledge of men and women, that at least 50 per cent. would be eliminated from the farm colony scheme on these two grounds alone.

The deaths from the slow, progressing forms of tuberculosis were over 36,000 in the year 1915. Taking as a basis for calculation as to the accommodation required, that only one-third are "cured" by sanatorium treatment, this gives a total of 12,000 patients to deal with annually. Allowing for 50 per cent. for women, and 50 per cent. of the remainder for reasons already given, as indicated, the yearly incubus of patients will be 3000, or, in five years, 15,000. Allowing for a minimum two years' course, it will need a very large farm on which to train 6000 students a year, and a still greater one to employ thousands of hands.

The next question to arise is; is it proposed to train the ex-patients as farmers, odd-job, out-door men, or as farm-labourers? Here it is desirable to compare conditions of living and wages. A collier, or a compositor, who has been accustomed to earning three pounds or more a week, is not going to be content with the wages of a farm-labourer or of an odd-job man; nor is he going to be satisfied with the latter's hours of labour, nor the conditions under which he at present exists. It is obvious, therefore, that when it is

intended to change a man's trade, it is essential to replace it with one of equal financial advantage.

Reasoning in this way, the collier, or compositor, under this scheme is bound to become a farmer. It follows that he has to learn a new trade. What length of time will it take him to acquire such a knowledge of farming that he can proceed from the colony and take a farm, and become self-supporting? That is the logical result of the farm colony scheme. It is essential to have land, and, in consequence, emigration is forced upon the training school. What number of town-bred men, having learned a definite trade, after the age of twenty-five, will be able to live on their earnings on a farm?

Is it really possible to make a successful farmer of a man who has been brought up in a town, learned a trade, and is over twenty-five years of age? If it is feasible, will there be enough land? Because each farmer will require about 250 acres, each year would give us, say, 2000 extra farmers, and in five years 10,000 farms would be required. It must not be forgotten they will not be allowed to enter Canada or Australia. As the work on farms is seasonable, I take it that at least two or three years would be necessary to train a farmer even moderately.

Who is going to train him to become a farmer? A doctor, or a farmer?

Here we come to the crucial medical point. Before training is begun, the man should be in good health, so that his tuition may go on uninterrupted by any doubt as to his capacity for work.

I want to make this point absolutely clear. There are at present three classes of tuberculosis institutions. One set relies on pure air and good food. Another, on these two adjuncts, *plus* a "little gardening," and there is a small minority who depend upon natural conditions of living, together with controlled inoculation, artificial or otherwise.

Those few who have practised auto-inoculation, know what a powerful weapon is in their hands, and the care necessary in its exhibition. It requires constant attention to see that some patients do enough work, and that others do not perform too much; discretion is required in the selection of a grade of exercise on which it is impossible for harm to result; the one object being to bring about the patient's recovery.

Anyone who has done these things is acquainted with the difficulties of restoring the patient's health and physical strength. To add to this strain on the medical man, a complete knowledge of farming and the burden of trying to find the patient work that will also train him, is too much to expect, even were it possible to do so.

Take, for example, the times usually spent on grades:—

One week on small basket work;
One on large basket work;
One on small hammer work;
One on large hammer work.

All these one-week grades are of the utmost importance, not only to produce auto-inoculations,

but to test the patients for transference to higher grades.

What sort of trade is it possible to teach in a month, with the grade of physical effort varying each week? Next comes a period of three weeks—almost the most important of all. It is the link between light work and hard work. The patient must be absolutely stabilised in his efforts on this grade, to avoid excessive inoculation.

Lastly, there are the final grades of hard work. Here, again, it is more than ever essential that the patient should PERFORM the hardest work, both to inoculate, and to test himself. Therefore the work should be such that it is only possible for him to maintain the hardest efforts. If he is learning a trade at the same time, it is almost impossible to keep up to this standard. It is not possible to do two things at once is an old adage.

If treatment and training are mingled, the treatment is spoilt, as already pointed out, and in addition, the patient's mental outlook is fogged, which should be fixed on an endeavour to regain his health, and not on learning a new trade.

Let us suppose that each tuberculous patient requires six months' treatment. What occupation can he be taught in that time, which will enable him to enter the trade union of that particular craft? Why then vitiate his chances of recovery, when you really can do practically nothing to help him to earn his living in that time?

I hope that I have now established the following points: (1) That the patient should be treated in a sanatorium before entertaining any idea of

training him; (2) That, even granting a farm colony is a sound scheme, it will not be available for or utilised by all ex-patients; (3) That considering the potential number of available farmers, there are not enough farms for them in this country, and that tuberculous emigrants are barred in the Colonies.

There is a final reason why any farm colony or other colony scheme is foredoomed to failure. Even if the authorities mix up treatment and training for a time, events will force them to see their errors when a scheme of any magnitude is inaugurated. It is easy for persons to say, "Now, look at my little colony! All goes well with those I train and treat, come and see us." I grant all this. It is possible to collect even 500 tuberculous patients who would be only too willing to enter these places, because of the difficulty of their retaining health and wealth in the ordinary way.

When, however, a comprehensive campaign begins its battle with tuberculosis, the matter will assume an entirely different aspect. I want to emphasise the point that the whole future success of any scheme should not be prejudiced by efforts made with only small bodies of men. Comprehensive after-care schemes for tuberculosis will mean great crowds of men, and that is where the conditions change.

As has been said, a patient must, first of all, be treated in a sanatorium. There he is taught to live a wholesome life, to regain his health by his own efforts, and, finally, made capable of the hardest work.

To obtain these results, the strictest discipline is necessary. Many patients before the war resented undergoing this discipline, to which they were unaccustomed. They seem now, after any war service, to resent it even more. The necessity for discipline in a patient's life will be apparent, if the question of auto-inoculation, or a patient's physical efforts, is properly assessed in the relative value of treatment. This discipline cannot be relaxed, and only the obvious improvement in their health makes the patients "stick out" their term of treatment.

Again, apart from all auto-inoculation or tuberculosis, wherever you have a body of men or women living under one roof with a Medical Officer or Governor in LOCO PARENTIS, it is absolutely essential to have discipline and rules; this is usually called "institutional life." After six months in a sanatorium the average patient is "fed up" with institutional life in general, and he wants to "clear out of it," and regain his freedom.

At the time, he does not consider his future perhaps, in the way that he will afterwards wish he had studied it. He has recovered his health; he KNOWS he can do hard work, and he would sooner "take a chance" at home than live in an institution. If I were in his place, I should do likewise.

This point requires the most careful thought. Is it likely that a man will submit to live in an institution, if he considers he has a dog's chance of life and work outside of it? I am not referring to the 66 per cent. whom we are not supposed to be

able to cure. A large percentage of these will stay anywhere for a time, but a great number even of these will go home rather than abide in an institution. How many of the 88 per cent. of patients who have recovered are going to remain? That is my point. If they are what I believe them to be, hardly one will stay on. There has already been trouble reported in the Press at one of these colonies, the patients leaving in a body, alleging bad food. I doubt if it was the food which "fed them up" but rather the institutional life, although they did not recognise it.

The very cases it is one's ambition to train will have none of it, and the material left will hardly stand the strain of hard work, even if it is trained; so that, eventually, it would become a large colony of light, out-door workers.

I have gone to this length in destructive criticism, because I desire to build on sure foundations. The scheme I am about to describe, will, I am certain prove practicable with the average tuberculous patient of intelligence. It will be called impossible, or Utopian, according to the mental attitude of the critic. But if any successful scheme is to be started to rid us of tuberculosis, something on the lines indicated is essential.

My scheme has the merit of being possible and practical in dealing with the great numbers of persons that must at the outset be considered.

What, then, are the problems to solve? They are, as I see them, five in number, and they concern the future of all patients after their discharge from treatment.

(1) The ex-patient with arrested disease, sputum, if any, not infectious, and having a suitable occupation and home conditions to which to return;

(2) The ex-patient with arrested disease, sputum, if any, not infectious, but having no suitable occupation to which he can return;

(3) The ex-patient with infective sputum who has arrested disease;

(4) The ex-patient who has not fully responded to treatment, but yet has some time to live; and

(5) The dying ex-patient.

The test for a patient to be classified as non-infectious, should be made through the agency of a guinea-pig.

There is no reason why the first class should not all return to their homes.

If only the outlook on tuberculosis could be changed, the majority of cases would, in my opinion, be in the first and second classes, so far as their health is concerned. But, unfortunately for the population of this country, the majority are in classes three, four, and five, the reason for this being that the earliest signs of the disease are not recognised soon enough, and even when they are, the most efficient treatment is not available for all.

The second class is a difficult one to deal with. Its chief obstruction are trade unions. It is possible to teach an ex-patient to be quite a handy man as a carpenter, but he is not a "carpenter" unless he has served his apprenticeship, and he will not be allowed to work as such

with other craftsmen. This same objection applies to many other trades. If suitable means can be evolved to overcome these difficulties, the solution is obvious.

I foresee that I shall be told that boot-repairing is a suitable employment for ex-tuberculous patients. This is by no means an ideal trade. It is only necessary to watch a man who spits in the street, he frequently obliterates his expectoration by spreading the sputum with his boot. The more worn the boot, the greater the opportunity for the sputum to find a place between the soles. The boot affords ample opportunity to dry the sputum. In ripping off the old soles, the worker is therefore exposed to the dust and consequent infection.

Many patients of the second class will have to be considered with the third and fourth classes. For these I advocate the building of a Garden City. Nothing at all in the nature of institutional life should be found there. A man should be permitted to do as he likes, so long as he conforms to the ordinary rules of the community. The only special regulation that the City would find it necessary to enforce, would be one to be dealt with later, concerning the limitation of infection.

In this special Garden City for Tuberculosis, there should be churches, schools, cinemas, public houses—everything in short, to make the place attractive. The churches and cinemas should not be, as they are in most places, unventilated, dark abodes for germs (the tubercle bacillus loves darkness). There should be no stained-glass saints in the windows of the churches, and the windows

should be *made to open.* Saints look incongruous when out of the vertical. There should be means to let in sunlight into cinemas when not in use. There should be cottages for tuberculous men to live in with their families. Many medical men hold that it is not right for children to live with a tuberculous parent for fear of infection. The matter has elsewhere been dealt with from that point of view. There is, however, no getting away from the fact that they will live together whether in or out of a Tuberculosis City, blood being thicker than water. There should be lodgings on ideal lines, for both the single men and women. These would, in my ideal Tuberculosis City, be built on the model of an hotel, *i.e.*, with single bed-rooms, and public sitting-rooms.

I would have numerous hotels, rather than large barracks, holding up to, say, 150 rooms, as a unit; and the hostels for single men and women would be separate. There would be also hospitals, and an attractive home for the dying. Around the City would be established factories on hygienic lines, where all workers could earn their living.

I will indicate only one or two suitable industries:—

Chair-making.
Box-making.
Leather-work: boots, bags, etc.

That is, those industries where machinery is employed, and the chief training is to instil into the workman's brain the method of operating a

particular machine. Factories or shops would have to be established for all who had skilled trades, such as, for example, printers, bookbinders, etc.

In October 1917 I attended a meeting at the Ministry of Pensions,* at which I was asked to state the suitable industries for ex-tuberculous patients. I told the Committee that it was much more feasible to indicate those that were not suitable.

There are three trades in ordinary life, as apart from this new City idea, which I would never advise an ex-patient to continue; yet these three trades could be successfully undertaken by ex-patients in the City of Tuberculosis. That being so, it clearly follows that most trades are suitable enough for ex-patients. It is the CONDITIONS UNDER WHICH THE TRADE IS CARRIED OUT THAT MATTER, NOT THE TRADE ITSELF. Therefore one of the greatest difficulties in the way of ex-patients becoming productive wage-earners, may be overcome by a scheme such as I suggest.

The three impossible trades for ex-patients, already mentioned, are:—

House-painting.
Bar-tending.
Laundry working.

All three trades are popularly accepted as very unsuitable, but from entirely erroneous reasons. Painters are said to become infected through the lead used in the paint. The truth is, that whenever there is a filthy house to clear, full of divers disease germs, the painter is the tradesman who has to

* *See* note on page 187.

undertake the work, and breathe the resultant dust. In the City of Tuberculosis, by education and inspection, this filth would be largely avoided, and the Medical Officer in charge would see that proper steps were taken to protect the ex-patients in their work of reparation.

Barmen are surmised to contract tuberculosis from the alcoholic liquors they drink and serve. The true explanation, however, is overlooked. It may be assumed that many persons suffering from tuberculosis frequent public houses, where the possible presence of spittoons does not prevent a large amount of their sputum reaching the floor—that is why the sawdust is put there. Further, public houses are generally ill-ventilated, and being kept shut all night, afford ideal conditions for the drying of the sputum. Consequently, the barman who sweeps up in the morning is almost certain to swallow or inhale germ-laden particles of dust. All this could be avoided in a properly conducted City. I have gone into details here, to illustrate the point that it is not the occupation but the *modus operandi* that counts.

The laundry workers, finally, give the most typical example of all. To the steam-laden atmosphere is accredited their disease—not to the obvious cause, which is the germ-laden handkerchiefs and sheets sent to be washed. All such articles in the Tuberculosis City would be dealt with as they are in a tuberculosis institution. It is clear, therefore, that all these three classes of trade could be usefully adopted in the City.

Certain ex-patients in class four would be able

to work as the former class, whilst others could not stand a full day's work. I would overcome this difficulty by dividing one man's work into two shifts, and letting two men perform it, providing other suitable jobs could not be found.

Another great advantage of this City would be that there would be no hesitation or prejudice in the use of sputum flasks in public places. In fact, it would be like a properly conducted sanatorium, where the patients very rapidly "tell off" any fellow-patient committing this unpardonable offence. Once let patients understand, as they do in a sanatorium, what promiscuous spitting means to them, and they will run no risk. So would it be in this ideal City—the citizens themselves would see to it.

But there will always be impossible persons to be dealt with, and means *must* exist to deal with such offenders. A special law would have to be enforced in the City to control those persons who would not properly dispose of their sputum. It would be necessary to have compulsory powers of detention for the offenders similar to those conferred under the Lunacy Acts, *i.e.*, the offending person, on being reported to the Medical Officer, would be brought before Justices and the case decided, as is done with mentally deficient persons. These are the persons who require segregation, and once it became known what trouble promiscuous spitting by persons who have been educated in the care of their sputum will lead to, it would have a most salutary effect in stopping the habit. I think legislation on these lines

would be acceptable to the country, so long as it is understood that there is no desire to interfere with the liberty of those persons who are reasonable, but only with those who are not.

After all, it is comparable with lunatic asylums in the past. The patients were detained chiefly on the grounds of prevention of harm to their fellows. An insane patient is detained because he *may* harm his fellows, and very few at that. A man with tubercle bacilli in his sputum can harm many persons if he is careless; why should he not be treated on similar lines to the mental case? He is wilfully doing wrong, whereas the lunatic is not responsible for his actions. It is only because the spreading of tuberculosis is unseen, and therefore unrealised, that this state of affairs exists to-day. It would not exist in the Tuberculosis City, because the citizens would have been educated in self-protection.

I know the objections that will be raised—firstly, the cost of the scheme; secondly, that ex-patients would not go to live in such a City.

To take the second point first; I am convinced that ex-patients would willingly do so. An ideal opportunity exists to-day, with the shortage of houses, to build such a City. The chance of getting a decent house in a decent place would in itself prove an irresistible attraction. As far as cost is concerned, I see no reason why a nucleus might not be formed around one of the specially-constructed munition factories, utilising the huts discarded by troops for a beginning. The cost also must be considered from two standpoints:

the increased production to the State from the exertions of persons who otherwise would have died, or who would have been unproductive, and the fact that the City might be made nearly self-supporting. But, as it is certain to be said that it would not pay its way, let us grant that it will be a burden to the State. If it is a success, and we see voluntary segregation and happiness entwined together—What is THAT worth to the State? SURELY IT IS WELL WORTH RUNNING SUCH A CITY, EVEN AT AN ABSOLUTE LOSS.

LIII

THAT IT IS DANGEROUS TO VISIT A TUBERCULOSIS HOSPITAL

From what has already been said, this is clearly untrue. I think that the necessary conditions for tuberculous infection frequently remain unconsidered:—

(1) There is the amount of infection or DOSE. Twenty tubercle bacilli will probably not cause infection unless (2) the VIRULENCE[77] is very high;

(3) But twenty bacilli of a low virulence may infect if the dose is REPEATED often enough. Where proper precautions are taken, the possibility of infection is nil.

A properly conducted tuberculosis institution

is much safer, as far as tuberculosis is concerned, than any other place.

LIV

THAT ANY SPITTOON IS A GOOD SPUTUM POT

The R.A.M.C. during the war supplied sputum pots for hospital use in France, which were not only obsolete but dangerous from a tuberculosis standpoint. I have already mentioned the large percentage of nurses who came as patients to Frimley from the London general hospitals. In the Navy, the greatest number of persons invalidated from tuberculosis are the sick-bay attendants. The incidence of tuberculosis in the Navy for the five years ending 1913 amongst the various categories, shows that the sick-berth staff suffered to the extent of 6·43 per 1000 of average ratio borne.[66] This is practically double the invalidity of the next highest rating, and almost three times the rate of the seamen—2·24 per 1000. I state these facts to emphasise the necessity of taking all possible precautions against infection.

The object of a sputum pot is a threefold one:—

(1) To collect the pulmonary discharges;

(2) To keep them moist until sterilised; and

(3) To keep the flies from carrying the bacilli to others.

The Army vessel fulfils the first but not the other two conditions.

The sputum pot almost universally used *outside* special institutions for tuberculosis is an ordinary mug with a spittoon-shaped top, thus :—

It is quite common to see lumps of sputum adhering to the spittoon-shaped entrance to the pot, as illustrated, where it soon becomes dry. I have observed flies apparently feeding on sputum so situated, and afterwards promenading on a piece of beef, awaiting consumption by a patient.

To prevent evaporation and flies distributing the sputum, all that is required is a lid.

A sputum pot must be operable by one hand, otherwise the patient has to sit up in bed to remove the lid. The lid must be detachable for cleaning purposes. A sputum pot fulfilling all these conditions is that illustrated on p. 204.

An ideal sputum flask does not exist. The best design is that of Dettweiler.* Every ambulant patient should be provided with one of these utensils, but this is seldom done, as the following figures will show.

Dr Sutcliffe, of the Ipswich Sanatorium, was so much impressed by the number of tuberculous patients arriving without flasks, that he culled the following statistics in November 1918.

The patients, be it noted, were arriving from Navy and Army hospitals, and from Insurance Committees, advised by medical men.

Of the patients, 93·75 per cent. had sputum before admission. In 57·77 per cent. bacilli had been found previous to admission. Only 8·88 per cent. of those with sputum arrived at the sanatorium with a flask. People still wonder how tuberculosis is contracted.

* This flask is designed on the principle of the unspillable ink-pot, which prevents the sputum adhering to the top, and consequently falling on the floor when the lid or stopper is moved during use. There is not in Great Britain to-day a sputum flask obtainable free from this obvious fault.

LV

THAT CARBOLIC ACID KILLS TUBERCLE BACILLI IN SPUTUM

There is no doubt that carbolic acid will kill tubercle bacilli if it comes in contact with them. This inhibitive action does not occur in sputum. The sputum is coughed up in pellets, which are not penetrated by the liquid. Carbolic acid is useful as a medium for rendering moist the receptacle for the sputum. Water equally well answers the purpose, from a bacteriological standard, as it is only necessary to keep sputum wet until its infectivity is destroyed. Water, to a lay mind, cannot achieve any such object, therefore it is best to direct patients to use some form of antiseptic fluid until the sputum is rendered innocuous. The carbolic acid will, of course, disinfect the inside of the collector, but it is better to immerse the sputum pot in boiling water to be absolutely safe.

LVI

THAT SPUTUM SHOULD BE BURNT

The known facts concerning the spreading of infection by sputum are as follows :—

(1) It is wet when expectorated ;

(2) It is not dangerous to handle so long as it is wet, as the bacilli cannot move, until

(3) When it is dry, the bacilli are free, and can be activated by currents of air, and so dispersed.

There are two methods of safely disposing of sputum whilst it is in a liquid state :—

(1) Through sterilisation by heat ; the bacillus is killed before boiling point is reached,[75] therefore, so long as the sputum boils for a second, the desired effect has been achieved ;

(2) By emptying the sputum into a water-carriage system of drainage.

I have heard it suggested that the latter is a bad practice, as the sewage thus becomes infected. To this, there is the answer that many patients pass tubercle bacilli with their fæces, and that the fæcal material thus passed by patients suffering from tuberculosis of the intestines has been emptied, unsterilised, down the drains for centuries.

Boiling is, however, the better method, because the vessel which contains the material can also be rendered sterile at the one operation. Thus it is perfectly safe to handle sputum in either manner.

If the burning of sputum were always undertaken by trained persons, there would be no possible objection to it. The danger lies in the careless person throwing two or three ounces of sputum on a dying fire. Much of it may run into the ash-bin, where it will be dried without sterilisation.

Muir and Ritchie[67] writing about tubercle bacilli, say: "When completely dried, they can resist a temperature of 100° C. for an hour. . . . Exposure to 70° C. in the moist-condition is fatal."

To instruct patients to burn their sputum, implies the use of a hazardous method, when a perfectly safe one is available.

LVII

THAT GOOD HOUSING WILL ERADICATE TUBERCULOSIS

Good housing alone will do no such thing. A perfect abode will not prevent a man being infected outside of it, nor will it afterwards prevent him from contaminating others within his domicile. It is a very narrow view to take, that any one thing will eradicate tuberculosis. Of what use is a good house with a bad tenant? It will neither cure nor inhibit tuberculosis. A patient must first be educated in hygienic methods of living, in cleanliness, food values, proper clothing, open

windows, etc., before the ideal home conditions will benefit him to their full extent. At the present time, the law can compel the landlord to maintain the house in good order, but there are no similar powers to compel the tenant to be a good one.

In the Report of the Welsh Memorial for 1916, one of the tuberculosis officers writes as follows :—

"During the year there has been a steady increase in the number of workmen's cottages. New streets have been added to, and the Garden Cities have grown considerably. One Garden City which I have in mind, is a good example of the modern colliery workmen's village. This is situated on a plateau with a grand prospect in all directions, and open to all the winds of heaven. Sunshine and pure air are unlimited. The cottages are in pairs on wide streets, with ample gardens at the rear. The houses, if kept clean, ought to be pleasant habitations, but it is not cheering to think that many are already very dirty, and have been infected by advanced cases of tuberculosis, which have been introduced from all parts of the United Kingdom.

"Family history, from a health point of view, is not enquired into, when miners are needed for a growing colliery, and the invalid is installed with the healthy in the new cottage in the Garden City. In this way, it has happened during the past year that infective cases with cavitation and free expectoration, have existed for months unknown to the health authorities, or to any medical man. This hopeless state of affairs may go on, and the fairest of Garden Cities become a collection of infected dwellings."

Filth, with closed windows, is not a good housing condition. We require education as well

as better homes. There is no greater educator in this respect than a well-conducted sanatorium. The patients there learn that they do not catch cold from open windows, and that instead they feel better for such conditions.

The open window is only one of the lessons learned at sanatoria, but it is one you can preach, and preach, and preach, but you will not be believed, until the individuals have actually tried it and realised for themselves the great benefit they receive from pure air.

The use of the sputum flask also affords an excellent illustration that "practice is better than precept." It was the custom of the tuberculosis officers in Wales to present to every patient diagnosed as tuberculous, a flask in which to collect the sputum. Full instructions as to its necessity and mode of usage, both verbal and printed, were given at the same time. For purely domestic reasons these flasks were taken from the patients on their admission to sanatoria, and one belonging to the institution substituted in its place. After a suitable greeting, the nurse admitting the patient would therefore ask each individual for his flask. The majority had to unpack their baggage before they could produce it! In the sanatorium the patient soon has an optical demonstration of the flask, and in a few days is quite at home with that vessel so much disdained by the public.

Proper habitations are a necessity, but are not er .dicators. There is an utter want of imagination in the idea that housing is all-important. Tuber-

culosis will not be wiped out by any single measure —neither by Sanatoria, Tuberculosis Institutes, Health Visitors, Education, Housing, nor Tuberculin. It will demand a mobilisation of all forces, medical as well as social, and even then it will prove a great and difficult problem.

LVIII

THAT PERSONS SUFFERING FROM TUBERCULOSIS SHOULD BE SEGREGATED

I would certainly agree with this if the definition of a dangerous case, as already given in this book, be accepted. But here I only treat the subject from the way it is usually regarded, namely, that the large majority of persons suffering from tuberculosis should be confined. There is a great deal of misconception concerning "dangerous cases." Many persons regard the dying as dangerous, and not the ambulant persons with one or two ounces per day of sputum containing bacilli.

The actuality is that the former swallow most of their sputum, to their own possible hurt, whilst the latter distribute their expectoration, to the probable detriment of others. It is necessary to point out these facts in order that it may be realised how difficult the task will be in deciding

what is a suitable case for segregation, where many are to be confined.

If there is to be any widespread segregation, such as is planned in many minds, it is necessary to consider the mentality of the patient who suspects he has tuberculosis, and his dread of a diagnosis, for fear of his segregation. He does not know whether he will be classed as "dangerous" or not. But, being human, he will not be caught, if he can help it, and in the meantime he will certainly be dangerous, as he will have no knowledge of preventing the spread of infection to others, and will therefore scatter it broadcast. Once again, no patient suffering from tuberculosis, whilst *compos mentis*, need be dangerous at all. I must repeat that a properly conducted tuberculosis institution is the safest place to live, so far as tuberculosis is concerned. It is hard enough now to induce patients to consult a medical man when there is an apprehension of tuberculosis, for fear of a positive diagnosis. It will be a thousand times greater if there is added the possibility of loss of freedom.

One of the developments of any widespread segregation would be later, rather than earlier, diagnosis. I do not, in any case, think widespread segregation is necessary, as I have said elsewhere.

Finally, should we as a nation agree to the possible loss of our freedom? Thousands of soldiers have been discharged from the Army suffering from tuberculosis. Practically all of these men either contracted it on active service, or had the latent disease stirred up by the hard-

ships of trench life. Is their reward to be confinement for life?

A man does not contract tuberculosis because of any misconduct, but because of the carelessness of others; why, therefore, this terrible punishment superimposed on the malady? Why not segregate all dangerous cases of venereal disease until they are free from infection? I know one answer is, that it would mean concealment and dissemination of the disease, just as I have said of tuberculosis.

LIX

THAT THE DEATH-RATE FOR TUBERCULOSIS IS FALLING, THE DISEASE DYING OUT, AND SANATORIA ARE THEREFORE UNNECESSARY

The following statement was made by Dr Wilbur,[69] chief statistician of the U.S.A. Census Bureau, at the International Congress on Tuberculosis: "If the curve representing the death-rate in Massachusetts from all forms of tuberculosis be produced according to the rate of decrease shown from 1881-1906, it will intersect the base line of the future, at the point of no deaths, about the year 1935; and the curves for England and Wales, and the United States (Registration area) will arrive at the disappearance of the disease some ten years later." The writer proceeds to say that

he himself does not state that this will by any means follow.

This very clearly illustrates the attitude of mind of those people who comforted themselves with the pre-war statistics of this pestilence. Unfortunately, since then, the death-rate from pulmonary tuberculosis has risen from 1084 per 1,000,000 persons living in the period 1912-14, to 1250 in 1917,[99] the rates being for civilians only. The Registrar-General points out war causes for this increment, which are referred to in detail later.

This is another very probable explanation of this expansion in the mortality rates, other than can be entirely associated with the war.

Since 1912, various schemes for the prevention and treatment of tuberculosis have been, more or less, actively advocated and adopted. It is quite probable that, apart from any good they are doing to individuals, they have brought about better diagnosis, and, consequently, a more accurate classification of deaths. Should this prove to be the case, the mortality rate should continue to rise until war influences have ceased, and even then it should not show such a favourable decline.

The increase in the tuberculosis mortality had begun in 1914, and the greatest increment was in 1915. These figures obviously represent deaths, and *not* the number of persons afflicted. Tuberculosis is a very lingering disease, and even allowing for an increase of a very active form, it is not in agreement with the fairly well-established course of the disease, that the conditions created by the

war during 1914 should have caused 41 civilians per million to have either contracted the disease and to have *died* from it in five months. It is usually estimated that the average length of life of a person with pulmonary tuberculosis, is from three to five years. It does not, therefore, seem reasonable to suppose that this increase was due to the war. More especially is this so, because the effects of the war were little, if at all, felt at that period. During the year 1915, although food problems had not yet begun to press hardly on the population, the deaths per million had risen by 175 over the number in 1913, or, in full numbers, 4819 extra deaths occurred.

Table[70] *showing Mortality from Tuberculosis of persons per million living, during the following years:—*

Year . . .	1912.	1913.	1914.	1915.	1916.	1917.
All forms .	1366	1340	1361	1515	1529	1624
Increase over former year .	...	−26	+21	+154	+14	+95
Pulmonary .	1039	1004	1045	1161	1178	1250
Increase over former year .	...	−35	+41	+116	+17	+72

Even allowing for war conditions, it would appear unlikely that the type of the disease had altered to such a degree that a large number of persons were dying much more rapidly than usually obtained.

According to arguments already advanced, it would follow that causes operating to better diagnosis during the years 1912 and 1913, should

not manifest themselves so far as deaths are concerned, until the years 1914 and 1915 and later. As has been stated, the war causes operating to produce tuberculosis during 1914 and 1915 are almost negligible. It is, therefore, quite as justifiable to suggest that the chief cause of the great increase of deaths in 1915 was due to better diagnosis, whilst the increase in 1917 was due to war causes during 1915 and 1916, as well as to better diagnosis. In other words, causes producing tuberculosis do not fructify as regards the resulting deaths, in any considerable numbers, for about two years, and then they continue for an indefinite period of time.

When the tuberculosis service in 1916 and afterwards, began to suffer more and more from loss of personnel, the death-rates did not increase to anything like the same proportion, even although the war conditions, especially with regard to food, were pressing hard on the civilian population. Whilst everyone will admit that the war has had a direct influence on a disease like tuberculosis, I certainly think that better notification has had much to do with this increase in the mortality rates.

Dr Stevenson, in his review of the Statistics for 1917, directs attention to the commandeering of asylums for hospital purposes, with the resultant over-crowding of all remaining institutions, and the probability of this having caused the increased mortality from tuberculosis in such places. The number of deaths from tuberculosis in asylums [71] was as follows :—

Years.	Deaths.	Years.	Deaths.
1912	1741	1915	2392
1913	1889	1917	4189
1914	1871		

Dr Stevenson adds: "It is noted that over-crowding had much abated in 1917 from what it was in 1915, when the deaths from tubercle were far fewer."

This is precisely the result one would expect. Over-crowding in 1915 meant the lowering of resistance in all concerned, and increased facilities for the spread of infection, and therefore an increased number of persons suffering from tuberculosis. But these, according to my argument, *would not die during* the year 1915, although that was the period of the war which had sounded their death-knell. They would be much more likely to succumb during 1917 than in 1916, or even later than the former year. In other words, there were practically no war causes operating on the civil population during 1914 and 1915 such as one would expect to produce tuberculosis, and in any case, any such elements would not show in results as deaths in any number until 1916.

Thus it is quite conceivable that increased facilities of diagnosis and treatment are about to demonstrate that our death-rate from this disease is much higher than it has been generally supposed to be.

55,934 civilians, and over 1173 sailors and soldiers, died in England and Wales during 1917 from tuberculosis, and there would be thousands

of persons more or less crippled. Is there any better plan to perpetuate a "C 3" population than to be content to leave a problem like this alone, because tuberculosis MAY die out of itself?

We are an old-fashioned people. That which has been with us for centuries disturbs us little, or we should not be so passive in this matter. Tuberculosis is a preventable disease. Supposing Great Britain was practically free from it, and a weekly ship-load of 1000 persons suffering from it, or 52,000 a year (which is approximately the annual mortality) arrived in the Thames. What would we do? We should take every step necessary to see that the disease was not permitted in any way to take root, much less to flourish in our midst—just as we have countered the plague in Asia.

One of the chief reasons urged against a more active campaign is the cost of carrying it out. Especially that of buildings for treatment, because in —— years they will not be needed. Supposing that there were to-day 50,000 sanatorium beds with no patients to occupy them, would anyone regret the money spent on them?

If it is vitally important not to have a "C 3" population, what does the expense matter compared with the loss to the State of thousands of "A 1" lives per annum.

Pulmonary tuberculosis commences to take its heaviest toll after the educational period of life has passed, and continues to impair the efficiency of the population by appropriating an increasing number of persons after they have learned their

trades. So that in considering the loss caused to the community, we must try to estimate

(1) The cost of production;

(2) The loss of output; under which heading must fall the number of years the workers would have been productive if their usefulness had not been curtailed;

(3) The maintenance of the non-productive citizens until they die.

The Chart 17, on page 220, gives a graphic representation of the havoc wrought by tuberculosis during a nation's most important period of man and womanhood.

In the United States, attempts by statisticians have been made to arrive at "the average cost in actual money of a death from tuberculosis." Dr Irving Fisher,[72] Professor of Political Economy at Yale, taking the average age for death at 37·6 years in males, and 33·4 in females, arrives at the figure of $8000, roughly, £1600,* or for the 40,000 deaths in England and Wales from pulmonary tuberculosis, £64,000,000 sterling, besides the cost of disability, and the unassessable cost of infinite personal pain and suffering and distress to the patients' friends and relatives.

* Pre-war.

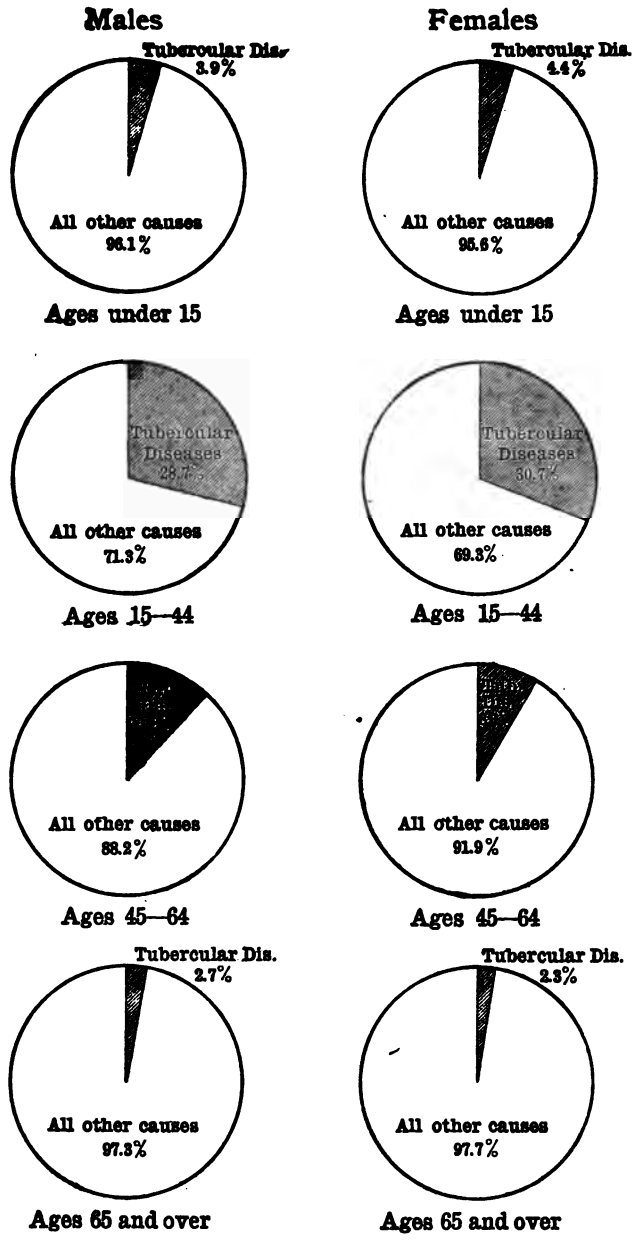


CHART 17.—Tabulation by The Prudential Insurance Company of America.

CONCLUSION

DURING the year 1916, 69,864 deaths occurred from tuberculosis in the United Kingdom, whilst the war, during four-and-a-half years, cost the Kingdom over 711,769 lives. A comparison between these two figures may be obtained by dividing the last numeral by 4·5, which will illustrate the average annual mortality from war causes. If, then, the resultant number is divided by the deaths from tuberculosis, the figure obtained is 2·26. This shows that for every 2·26 deaths caused by enemy action, 1·00 deaths were occasioned by tuberculosis. To exemplify the fact in another way, the Central Powers, with their utmost, unscrupulous concentration of effort to kill, only succeeded in destroying, in each year of war, as many lives as tuberculosis claims as its victims in every two-and-a-quarter years of peace. Tuberculosis is, therefore, to our race a far more serious enemy, because its activities cannot be controlled by treaties.

The atmosphere of war and the desire to win stimulated the efforts of all to combat new diseases, and to find effective protection from certain enemy actions. The work and energy expended on septic wounds is a wonderful example of the former,

whilst the latest gas-mask was an equal marvel of ingenuity to frustrate the latter. Why do we not in times of peace demonstrate an equal zeal to grapple with the greater enemy? Why this apparent apathy? Why should we combat war diseases and leave the more dangerous and invisible foe to its own devices? Possibly the factor that underlies the reason is that the ailment is so little understood by the majority of medical men.

To make this clear, I give the two following examples:—

(1) The fact (the medical profession entirely agree) that tuberculosis is a preventable disease. It is stated in this book that during the fifteen years I was connected with the Brompton Hospital the women workers were practically exempt from tuberculosis; whilst nurses from general hospitals contracted it, as do, also, the sick-berth men of the Royal Navy. Both these latter categories work under direct medical supervision, but not those specially trained with regard to tuberculosis. How, therefore, can we expect to prevent infection amongst the public, when the medical profession, as a whole, do not undertake effective measures to protect their own colleagues.

(2) That in 1907, Inman explained to me that I was unconsciously using auto-inoculation as a means of treatment. Wright had several years previously enunciated his theory. I have, therefore, had the treatment of tuberculous patients both before and since I was aware of the principles of auto-inoculation. The difference between the

two periods resembles that between night and day. In 1908 both Inman[73] and I[74] published papers on Treatment by Auto-inoculation. So far as I am aware, no one has publicly refuted what was written eleven years ago. Here, then, the researches of Wright and Inman led to something practical, namely, that in fever due to tuberculosis, it is better to keep patients completely immobilised, in bed, than to allow them to get up, even to evacuate their bowels, and that auto-inoculation was a means of treatment. Research led to these new ideas, proving, I consider, the theory to be sound. Is auto-inoculation being taught in all the medical schools? So far as I know, it is not. What does this mean in plain English? It means that medical men begin to practise their profession without having been thoroughly instructed in tuberculosis.

As is known, few general hospitals admit cases of pulmonary tuberculosis. There is no compulsory special course on the subject for their students. Is it, therefore, to be wondered that these shibboleths exist? That patients are being sent to sea, or to the sea-side for treatment, for a disease in which this process occurs, and which requires the most careful adjustment?

This state of affairs will continue until there is a head to the medical profession, which will consider all the information collected by the various workers, and *direct* what must be taught in the medical schools. I have great hopes that the new Ministry of Health will eventually prove to be this missing head.

I have already referred to the wise and repeated demands for research, but I would again emphasise that, in the meantime, steps should be taken to utilise clinically the knowledge already accumulated, and which is at present, except by a minority, utterly neglected. Otherwise, of what use is research if the information gleaned is to lie idle?

I would summarise such information as follows:—

That so-called "open air" treatment is not sanatorium treatment;

That "homes for tuberculosis" are not sanatoria;

That "a little gardening" is neither graduated labour nor auto-inoculation;

That auto-inoculation is a natural method of treatment;

That inoculation tests of sputum and blood should always be undertaken when microscopical examinations yield negative results;

That one or two tubercle bacilli negative microscopical examinations of sputum prove nothing.

That practically all cases of pleurisy in young persons are due to tuberculosis, except when there is another obvious cause;

That inoculation of pleural serum may establish a diagnosis of tuberculosis;

That influenza is frequently the first indication of tuberculosis—if auto-inoculation were understood, this would be obvious;

That hæmoptysis in young persons is nearly always due to tuberculosis;

That it is better to test a tuberculous patient's condition by exercise, than to endeavour to ascertain his condition by other methods;

That climate, provided it is not actually unsuitable, has little if anything to do with treatment.

That it is not wise to fatten a tuberculous person unduly;

That the man with the sputum flask is a safer indoor neighbour than the man spitting into his handkerchief or on to the floor;

That milk products are as dangerous as milk;

That it is unwise to intermingle social matters with medical treatment;

That physical signs have not now the importance formerly attached to them;

That properly conducted tuberculosis institutions are not dangerous; and

Finally, it is absolutely proved that patients have definitely suffered from tuberculosis whilst their sputum has been repeatedly pronounced microscopically bacilli-free, and that, therefore, it can happen again, which means: That it is not necessary to find tubercle bacilli to establish tuberculosis. What, however, is the attitude of mind of the anti-sanatorium critics? It is, that if any persons do well after discharge from a sanatorium, it is because they did not have tuberculosis since tubercle bacilli were not found in their sputum.

When the above information has been absorbed by the whole profession, then the day will come when the majority of patients will be diagnosed before the disease has overcome their resistance,

and treatment will not then receive so much undeserved ostracism.

It has already been stated that Nageli has demonstrated that large numbers of the human race have recovered from tuberculosis without their being aware of their having had the affliction. This is an indication of a very high natural resistance to the disease, and it is a fair assumption that, if the problem were approached on the lines indicated in this book, the majority of patients would make good recoveries.

A proper appreciation of auto-inoculation by the whole of the medical profession, will be of infinitely greater value to the State than any one other measure in the eradication of this disease.

The new views enunciated by Mackenzie and Lewis concerning the definition of observations in affections of the heart, have unquestionably demonstrated that the value of the stethoscope has declined. The significance of a cardiac murmur has, fortunately, been properly appraised, to the everlasting comfort of many persons who would otherwise have been condemned to spend their lives unnecessarily as invalids.

In the case of the heart, it is not the competence or incompetence of a valve or valves that settles the efficiency or inefficiency of that organ, but the strength of the cardiac muscle to perform its function. So it is with pulmonary tuberculosis; it is neither the area of lung involved nor the absence or presence of cavities that concerns the interpretation of the disease, but the specific resist-

ance of the individual's blood fluids to the bacteria and their products.

These doctrines are not being taught in all the medical schools from a correct perspective. Too much attention is concentrated on the value of physical signs at rest, without regard to the established tests by graded exercises.

Until all the instructors of medical students appreciate the limitations of the stethoscope and the usurpation of its place by new tests, until then there will be little understanding of pulmonary tuberculosis.

Unfortunately, the stethoscope is not the only thing that is holding back progress. Keith in *Menders of the Maimed*, says: "I again bring you back to the bed-rock of Hunterian teaching; the only rational means of treatment are those which are based on the natural recuperative powers of the body." Nature's methods of healing pulmonary tuberculosis have been known for over ten years, but except by an unappreciable minority, they are not being used. Need I write another word?

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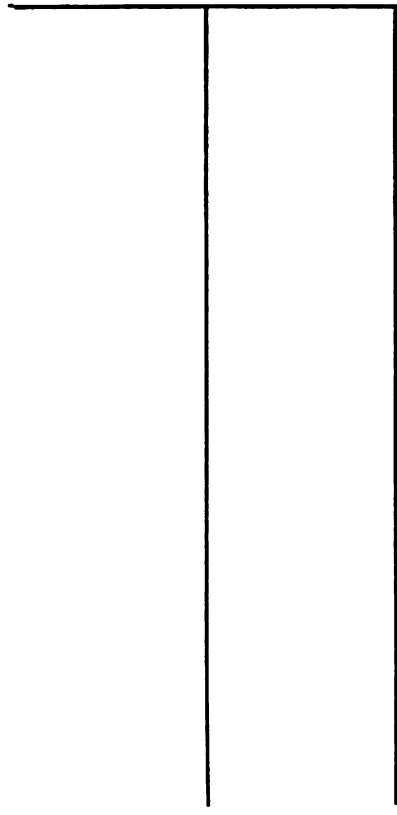

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